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CONTENTS OF PART III.—VOL. III.

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IV.—*An Investigation into the Power of Locomotive Engines, and the Effect produced by that Power at different Velocities.*

By PETER BARLOW, F.R.S., Cor. Mem. Inst. France, of the Imp. and Roy. Academies of St. Petersburg and Brussels, Hon. M. Inst. C.E., &c. &c.

My object in this paper is to make a comparison between the actual power expended by a locomotive engine, and the effect produced by that power under different circumstances, particularly at different velocities, and with engines varying considerably in weight and in evaporating power.

I am quite aware of the difficulty of computing the amount of *effective* power due to the piston pressure of a locomotive engine, or, which is the same, of correctly estimating the deductions which ought to be made for the several irregularities and impediments to which such engines are exposed; but the same difficulty is not encountered while we are only estimating the power *expended*, and it is therefore to the latter that I propose to direct the reader's attention. The method I propose to pursue in making this estimation is as follows.

Method pursued. If we know experimentally the number of cubic feet of water evaporated in an hour, or in any given time, by an engine, the space passed over in that time, the diameter of the driving wheels, the length of stroke, and the capacity of the cylinders—we hence know the number of cubic feet of steam which have been produced from one cubic foot of water, and hence, again, by the experiments that have been made on the force of steam by different persons, we know the pressure per inch on the piston; and then making a due allowance for the atmospheric pressure on the piston, the friction of the engine gear, &c., we have left the force which ought to be effective on the piston, and this being reduced to the circumference of the wheel should be equal to the resistance opposed by the load, which, on a level plane, consists of axle friction, road resistance, and the resistance of the atmosphere to the engine and carriages. Unfortunately, however, we know that the effective force is much less than would appear from this calculation, because we are thus assuming,—1st, that there is a uniform steam pressure, and that no steam is wasted;—2nd, that there is no skidding, and no priming; in fact, that the whole power expended, after deducting for the pressure of the atmosphere and friction of engine gear, is usefully employed, which is very far from being the case. Still, however, we

shall, at all events, know by such an investigation, what resistance we ought to be able to overcome, and by comparing this with the best estimate we can make of the actual resistance opposed by the trains, we shall learn what amount of power is wastefully expended, and knowing this, it may assist us in tracing the defects, and probably may lead to a means of remedying the evil.

The experiments I propose to avail myself of in this comparison, are selected from those given in Mr. Wood's Report to the Directors of the Great Western Railway (in which all the above-specified data are given), viz., those made on the North Star and Harvey Coombe engines, these being selected as exhibiting the effects on engines of the greatest and least power of evaporation, the ratio of their powers being as 100 to 56·84. To save the trouble of reference, such columns of the original tables as are requisite for our purpose, are given at the end of this paper, together with additional columns resulting from the computation first indicated. (*See* Table A, page 195.)

It will be observed that two different speeds are given, one, denominated "the mean maximum speed," being that which the engines made between the times when they first acquired their full speed, and the shutting off the steam from the pistons; the other, denominated "the mean speed," is that which is reckoned from the first starting of the engine to the final termination of its motion, but it is the last speed only to which the following deductions refer.

Construction of Table. It may be well, however, before we proceed, to give an example by way of illustrating the mode of calculation above referred to.

First, the *North Star* has 7 feet driving wheels, which may be called 22 feet in circumference, the stroke of the piston is 18 inches, and the diameter of the cylinder, 16 inches. From these data we have as follows:—

$$\text{The number of revolutions per mile} = \frac{5280}{22} = 240.$$

$$\text{The contents of each cylinder} = 3{\cdot}141 \times 8^2 \times 18 = 3618{\cdot}432 \text{ cubic inches}$$

$$= \frac{3618{\cdot}432}{1728} = 2{\cdot}094 \quad \text{cubic feet.}$$

Contents of both cylinders $= 4{\cdot}188$.

And cubic feet of steam per mile $= 4{\cdot}188 \times 240 \times 2 = 2010$.

In Experiment III. the mean speed is 33·9 miles per hour, therefore

Number of cubic feet of steam used per hour $= 33{\cdot}9 \times 2010 = 68139$.

Now the water evaporated in that time being, by the Table, 136 cubic feet, we have $\frac{68139}{136} = 501$ cubic feet of steam to 1 cubic foot of water.

To ascertain the pressure due to steam of this density, I have employed a Table given by Tredgold in his work on the Steam Engine, computed from the data furnished by Messrs. Southern and Creighton in 1802, from experiments made with great care, and with a view to obtain practical results. To save reference, so much of this Table as is necessary for the present purpose, is also given at the end of this paper (Table B, page 196).

Employing this Table, we find by interpolation that steam of this density has a pressure equal to 115 inches of mercury, or to $57\frac{1}{2}$lbs. per square inch, from which deducting 15 lbs. for atmospheric pressure, and 1 lb. for the friction of the engine gear, there is left $41\frac{1}{2}$lbs. per square inch on the piston, which, being the applied force, ought to be effective towards the traction of the gross load.

The area of the pistons being 402 inches, and the ratio of the double stroke to the circumference of the wheel being as 3 to 22, and the gross load in this experiment 70·6 tons, we have as follows:—

The power applied $= 41{\cdot}5 \times 402 \times 3 = 5229$ lbs.

The work done $= 70{\cdot}6 \times 22 = 1553{\cdot}2$ tons.

Then the ratio of the applied power to the work done, or the applied steam power per ton $= \frac{5229}{1553{\cdot}2} = 32$ lbs.

That is, if the whole steam power expended in this experiment could have been employed without waste, it ought, at the above velocity, to have been competent to overcome a constant retarding force of 32 lbs. per ton.

In a similar way the numbers in the last column of the Table have been computed.

Referring in the first instance to the result worked out above, viz., the third tabulated experiment, it will be seen that the steam power expended per ton of the gross load amounts to 32 lbs., whereas it has been hitherto commonly assumed, that on a level—which this line is very nearly,—the retardation does not amount to more than 9 lbs. per ton, so that there appears to have been expended a power more than three times greater than the mechanical resistance to which it was opposed, at least according to the views hitherto taken of the subject. And it is of the first importance to ascertain whether there is really this waste of power, or whether the resistance to the load is not actually much greater than has been commonly assumed; also whether this resistance is not greater with greater velocities, and if so, from what cause it proceeds. I have

in a subsequent page examined the question of resistance, from which I think it will appear that the resistance has been hitherto taken too small with high velocities; but after all, the great discrepance between the power expended and any estimated probable resistance overcome, is, I think, clearly due to a waste of power in the engine; for it will be observed that the experiment referred to here, is one that gives the most favourable result in the first set of experiments, for what may be termed high speed. The first tabulated experiment, which does not give a greatly increased velocity when treated in the same way, shows an expenditure of power amounting to 72 lbs. per ton, being eight or nine times greater than the usual admitted resistance.

In the smaller engine the power expended per ton does not appear so great, but neither is the maximum speed so great; it ought, however, to be observed that the latter experiments were made on a line having steeper gradients than those on the former, and consequently the traction per ton, including gravity on the ascending planes, was considerably more than that merely due to the friction of the load; so that it appears there was upon the whole much less power wasted here than in the former case. Now to what is this to be attributed? Is it due to the different proportions in the parts of the two engines, or to the difference in the velocities; or has such weight and power been given to the *North Star* as surpass what can be advantageously employed? These are questions which appear to me well deserving of serious consideration.

I have before observed, and I again repeat, that I do not offer the above calculations as strictly correct; yet if any thing definite be known of the power of steam, the results ought not to be far from the truth. I am aware also that I have made no deduction for the resistance of the blast pipe, which is perhaps considerable; I have seen it estimated at 3 lbs. per inch on the piston, and it may even be more, it may perhaps be greater than is really necessary. This again I conceive is another important question, particularly as it is obvious from what has been stated, that it is not the want of power in the engines at present employed, but some defect in making use of their power, which at this time limits their effective operations, more especially in the accomplishment of great speed.

ON THE RESISTANCE OF RAILWAY TRAINS AT DIFFERENT SPEEDS.

The resistances to which the engine and carriages of railway trains are exposed may be classed under the following heads:—

1. The resistance of the atmosphere.
2. The friction at the axles.
3. The road resistance.

So much has recently been said on the subject of atmospheric resistance that I am induced to consider this question first, and I shall again avail myself of Mr. Wood's experiments—namely, those made on the Whiston and Madeley planes.

These consist of the means of three sets of experiments, as follows, viz.—

1st. A train of four carriages weighing 15·6 tons, obtained a uniform velocity of 31 miles per hour on a plane descending 1 in 96.

2nd. A train consisting of the same four carriages loaded, weighing 18·05 tons, acquired a uniform velocity on the same plane of $32\frac{1}{4}$ miles per hour.

3rd. A train of four similar carriages weighing 18·05 tons, obtained, on a plane descending 1 in 177, a uniform velocity of 21 miles per hour.

In all these cases, as the motion was uniform, the accelerating force and the retarding force must have been equal, and the former being given by the slope of the plane, the whole retardation is known; and it seems only necessary, by a comparison of these results, two and two together, to separate the different effects from each other.

If the retarding forces above enumerated were all subject to the same law, we should not be able to do this; but, assuming that the resistance of the air varies as the square of the velocity, and that the two latter are constant forces, at all velocities, we can easily separate the effects by a comparison of any two of these results with each other.

First, then, let us compare the second experiment with the third, in which the gross weights are the same, viz., 18·05 tons.

We must here observe, that a part of the descending weight is a rolling mass, we must not, therefore, take the whole weight divided by the inclination of the plane for the accelerating force. To arrive at the actual acceleration we ought to know the weight of the 16 wheels and their axles; this datum, however, is not given, but we shall not be far wrong in taking this weight at about $2\frac{1}{2}$ tons, or as the experiments are only comparative, let us assume the weight at 3 tons in each case, and that the distance of the centre of oscillation, to that of gravity,

from the bearing point, is as 3 to 2, which agrees very nearly with Mr. Wood's experimental determination of these points, (*see* Wood's "Treatise on Railways," page 383). On these assumptions the accelerating force of the wheels being $\frac{2}{3}$rds of their weight, the whole accelerating force in the second and third experiments will be $\frac{17{\cdot}05}{96}$ and $\frac{17{\cdot}05}{177}$. Let $\frac{1}{f}$ denote the ratio of the friction, to the load *producing friction,* which we have assumed to be three tons less than the gross load; therefore, the friction, in both these cases, will be $\frac{15{\cdot}05}{f}$ (classing the resistance Nos. 2 and 3 under the general term friction).

Again, let Φ denote the force due to the atmospheric resistance on a unit of surface, and with the velocity of one mile per hour, then if a be taken to denote the opposing surface, and v', v, the two observed velocities, we shall have the following equations, viz.—

$$\frac{17{\cdot}05}{96} - \frac{15{\cdot}05}{f} = a\,\Phi\,v'^2$$

$$\frac{17{\cdot}05}{177} - \frac{15{\cdot}05}{f} = a\,\Phi\,v^2$$

$$\text{Whence,}\quad a\,\Phi = \frac{17{\cdot}05\,(177 - 96)}{177 \times 96\,(v'^2 - v^2)}$$

Now $v' = 32\frac{1}{4}$ and $v = 21$, and substituting these values we obtain

$$a\,\Phi = {\cdot}304$$

$$\text{Consequently,}\quad {\cdot}304\,v'^2 = 316 \text{ lbs.}$$

$${\cdot}304\,v^2 = 134 \text{ lbs.}$$

These results differ slightly from those of Mr. Wood, which arises probably from our having taken the weight of the wheels and axles too great; but as has been before observed, they are intended only for comparing the results with each other, and the difference is therefore of little consequence. In the article in question these are stated at 329 lbs. and 135 lbs. This is the only comparison made in the Report, and if we stopped here, it would appear to be confirmed by these computations; but let us now proceed exactly in the same way, and compare the first experiment with the second. Here the loads are 15·6 tons, and 18·05 tons; the parts of the loads subject to the friction 12·6 and 15·05 tons, and the accelerating forces $\frac{14{\cdot}6}{96}$ and $\frac{17{\cdot}05}{96}$. Our equations, therefore, now are—

$$\frac{14\cdot6}{96} - \frac{12\cdot6}{f} = a \; \Phi \; v^2$$

$$\frac{17\cdot05}{96} - \frac{15\cdot05}{f} = a \; \Phi \; v'^2$$

Whence we obtain, $f = 96 \times \frac{12\cdot6 \; v'^2 - 15\cdot05 \; v^2}{14\cdot6 \; v'^2 - 17\cdot05 \; v^2}$.

In this case $v' = 32\frac{1}{4}$, $v = 31$, and consequently, $\frac{1}{f} = \frac{1}{108\cdot5}$, which value substituted in the original equations gives

$$a \, \Phi \, v'^2 = 87\cdot1 \text{ lbs.}$$
$$a \, \Phi \, v^2 = 80\cdot64 \text{ lbs.}$$

That is, by comparing the second experiment with the third, we find the resistance of the atmosphere at $32\frac{1}{4}$ miles per hour to amount to 316 lbs.; and by comparing the first with the second (proceeding precisely the same way), we find the resistance at the same velocity of $32\frac{1}{4}$ to be only 87·1 lbs. The friction by the former comparison being only 5 lbs. or 6 lbs. per ton, and by the latter nearly 20 lbs. per ton.

Such discrepancies as these, where we ought to have a tolerably close coincidence, can lead us only to one conclusion, namely, that it is impossible with experiments conducted on these principles to draw any conclusions relative to the resistance of the atmosphere to railway carriages at different velocities; and the reason of this impossibility is sufficiently obvious; for, by referring to the above formula, it is plain that every thing depends upon the values of v and v', and these ought to express the relative velocities of the wind and of the carriages, whereas they denote, as above employed, the velocity of the carriages only, and cannot therefore be expected to approximate towards the truth, except in a perfectly calm atmosphere. While, however, we are inevitably led to this conclusion respecting the very unsatisfactory nature of deductions drawn from experiments of this kind, there can be no doubt whatever of the important fact, that the carriages did sustain, from one cause or other, the total resistance stated in the article in question; that is to say, a resistance equal to the gravitating power of the plane, that is, of 345 lbs. with the gross load of 15·6 tons, and of 397 lbs. with a load of 18·05 tons; being a total resistance of about 22 lbs. per ton in each case, at velocities of 31 or 32 miles per hour, and only about 12 lbs. at a velocity of 21 miles per hour. Other experiments lead

us to believe, that at velocities of 15 or 16 miles per hour, the resistance does not amount to more than 8 lbs. or 9 lbs. per ton ; others again give the resistance much less than these last numbers indicate, viz., not more than 5 lbs. or 6 lbs.

If this great difference of retardation were chiefly due to atmospheric resistance, we should indeed very soon arrive at the greatest attainable railway speed, because our contention would be with a natural force, which it would be in vain to endeavour to overcome, and difficult even to modify; but, if it only arise out of imperfect mechanical contruction, then we may hope that the same perseverance and energy which have already conquered so many difficulties, may enable engineers to modify and reduce those which at present remain to impede their progress of improvement. Let us, then, examine the question under another point of view.

The resistance to railway carriages consists, as we have seen, of three distinct forces enumerated in a preceding page, of which the first, whatever be its amount, we may consider as varying either exactly or nearly as the square of the velocity. The other two forces are generally assumed to be constant, and consequently, that they are independent of the velocity.

That friction is a constant force, has been proved by numerous experiments, and in none more satisfactorily than in those published by George Rennie, Esq. in the "Philosophical Transactions" for 1829. But, it must be remembered, that in experiments made with this view, the utmost care is always taken that all the machinery employed shall be in the most perfect order, the motions free and undisturbed, and every part of the apparatus immoveably fixed and steady. Now this is very far from being the case with a railway carriage, which is in a continual state of oscillation from side to side, and receives also constant blows and concussions from bad joints or other defects in the road. These cannot fail of bringing straining forces on the axles, and, unquestionably, increasing the amount of friction at those parts; and as these blows and irregularities are greater with greater velocities, we have every reason to conclude, that although in the most accurate experiments, friction is found to be a constant force under a uniform pressure, this law by no means holds good in respect to the axles of railway trains, the pressure on these varying every instant, by the cross strains and percussions to which the carriages are exposed.

With respect to the other retarding force, viz., the road resistance: this, while the motion is smooth and tolerably even, would also be uniform with any given radius of wheel; but this is, as we have seen, very far from being

the case, the effect being here even more obviously dependent on the velocity than in the case of axle friction. Not only is the impediment at each blow at a bad joint, or other irregularity, at least proportional to the velocity, but the number of consequent blows and percussions are also greater, as must be obvious to any one who will notice the difference in the motion at high and at low velocities in a railway carriage. There is also another species of retardation which must be included in the road resistance, namely, the effect of the deflection of the rails. This acts after the manner of an inclined plane, so that even on the most level line, the carriages are constantly, for half the length of the rail at least, ascending a plane; and the quantity of this deflection, and consequently the amount of this retardation, has been shown experimentally to be greater with greater velocities, as will be seen on referring to my Second Report to the Directors of the London and Birmingham Railway.

It may be altogether out of our power to determine the law according to which the friction and road resistance vary with the velocity; but that they are greater with greater velocities is unquestionable, and it is therefore quite erroneous to attribute that increase wholly to atmospheric resistance.

It may be said, that so long as the increased resistance is admitted to the amount stated in the experiments referred to, it is of little consequence from what cause it proceeds, but this I conceive is quite an erroneous view of the question.

If all the observed increased retardation with great velocities proceeded from the atmosphere, we should have an unconquerable impediment opposed to our future progress; but, if the retardation proceeds from the other causes glanced at above, then there is every inducement for engineers to endeavour by improving the construction of their lines, particularly the joints of the rails, in order thereby to overcome, as far as possible, the mere mechanical impediments which at present oppose themselves to the attainment of all the advantages of which the railway system seems to be susceptible. With respect to atmospheric resistance, our knowledge is certainly not so perfect as could be wished, and one reason is, that till the introduction of railways, we had no unobjectionable means of making experiments on a sufficiently large scale; but the velocities now attainable are such as would enable any person to arrive at very exact determinations on the subject, and it is much to be wished that some one would make the necessary experiments.

The best results we at present possess on the subject are those of Dr.

Hutton, as published in his "Tracts;" but the surface of the bodies experimented on were small in comparison with the frontage of a railway carriage or locomotive engine, so that it is very desirable we should obtain new data on the subject.

I may just observe here, that Mr. Smeaton published, in the "Philosophical Transactions" for 1751, a valuable set of experiments on the resistance of the air on windmill sails; and in the same paper he gave a table of direct resistances at different velocities, by a Mr. Rouse, adding his favourable opinion relative to its accuracy. These results, in consequence of the general practical nature of Smeaton's communications, have been, I believe, commonly considered as deductions from experiments and observations; but I find, on submitting them to the test, they are merely theoretical computations from an erroneous theory at that time entertained, that is to say, that the pressure of the air at any given velocity is equal to the weight of a column of air of double the height from which a heavy body must fall to acquire the given velocity.* These numbers are therefore not to be depended on; they are too high, exceeding considerably all those obtained experimentally by Dr. Hutton.

It is to be hoped, therefore, that some person who has the opportunity of doing it, will undertake, with proper apparatus, a set of experiments by which this question (by no means an unimportant one in railway practice) may be satisfactorily settled.

That the aggregate resistance is the main question is very true, but we are not likely to arrive at it in the aggregate, till we can obtain the separate effects; particularly as one part depends upon the surface exposed, and the others upon the weight of the load.

There can be no doubt that the aggregate resistance at velocities of 25 or 30 miles per hour is much greater than has hitherto been supposed, this is proved by the whole of the experiments already referred to; and that this resistance should be better understood is very evident, when we consider that upon it depends the great question of economy with respect to gradients. If, as has been generally assumed, friction was the whole resistance to be overcome,

* The formula from which these numbers appear to have been computed for any velocity, v, is $p = \frac{2a \times \frac{1}{9}}{16}$ lbs. for each square foot; a being the height from which a body must fall to acquire the velocity v.

There is certainly a trifling difference between the numbers thus computed and those in the table, but it is generally very inconsiderable, in most cases not amounting to the tenth of a pound.

and amounted to only 9 lbs. per ton, or $\frac{1}{250}$th of the load, an acclivity of 1 in 250 would double the tractive force required; and according to the probable traffic of the road, a greater or less sum might be advantageously expended in reducing it towards a level.

But if the aggregate resistance on a level be 20 lbs. per ton, or $\frac{1}{112}$th part of the load, then such an acclivity of 1 in 250 would be of much less comparative mechanical disadvantage, and the same sum could not be economically expended in the reduction. When we know the amount of total resistance in parts of the gross load at any given mean velocity, nothing is more easy than to estimate the increased tractive force requisite to ascend a plane of given inclination, and consequently the increased power that must be expended to ascend that plane; but even in this case errors have been made in the estimated effect. Suppose, for instance, a plane had such an ascent that the effect of gravity on it were equal to the friction on a level, and admit for the present that the friction remained the same on the plane as on the level, in this case the force of traction is obviously doubled, and it has in some cases been assumed that therefore the power expended in ascending the plane is doubled also; but this is not the case, the power expended consists of that which is necessary to balance the friction of the engine gear, and the resistance of the air to the piston—*plus* that which is employed in overcoming the friction, and it is only the latter that is double, the former remaining the same in both cases, and this constantly absorbed power, in the general run of engines amounts to full one-third of the whole power consumed; to reckon, therefore, that the expenditure of power is doubled because the tractive force is doubled in ascending such a plane as is here supposed, is estimating the mechanical disadvantage of the plane at much too high a rate.

Again, in descending an inclined plane, it has been sometimes assumed that whereas in ascending we had the whole power of the plane against us, so in descending it is all in our favour; and, excepting the absorbed power which is constant, this is true in theory but not in practice. Regard to the public safety will not allow us to avail ourselves in the descent, of the gravitating power of the steeper planes, and only of a certain portion of it in planes of less slopes. In others, viz., in planes descending 1 in 500 or 700 to 1 in 1000, &c., we may perhaps allow the whole gravitating power of the plane to act effectively, and our velocities, therefore, in the two latter cases to be proportionably increased.

I have had an opportunity of communicating with the engineers of several working lines, and have endeavoured to learn from them what acceleration is allowable in the descent of planes of different inclinations. I have thus learned that the practice is different on different railways; but forming a mean result from the whole, it may be stated that in descending with *heavy loads* planes of $\frac{1}{96}$, $\frac{1}{100}$, $\frac{1}{120}$, it is not safe to descend with a greater mean speed than is attainable with the same load on a level; and that on planes of very considerable lengths, varying from $\frac{1}{400}$ to $\frac{1}{500}$, the speed seldom is allowed to exceed that on a level, by more than one-fifth, but that on planes between $\frac{1}{750}$ and a level, the whole attainable speed is admissible.

It is, therefore, of no avail that we actually possess a power that would give us extraordinary rates of speed, if considerations of safety will not admit of our employing it; nor is it of any advantage that we can descend these planes with the admitted velocities, with less piston's pressure, because the steam which would otherwise escape through the cylinders is now wasted at the safety valves, so that we can claim for the advantage of the gravitating powers of the plane only so much as practical considerations will allow us to employ.

Founded on these facts I have, in the last edition of my "Treatise on the Strength of Materials," given a table showing the mean effect of gradients of different degrees, assuming the friction as 9 lbs. per ton. This is referred to by Mr. Wood in his "Treatise on Railways." It appears, however, that he has quite misunderstood the principles on which it is founded, as he states, that I have "assumed, in all cases, the descending effect to be the same as on a level." I am quite sure this mis-statement is wholly unintentional on his part, and that he will have much pleasure in correcting it in any future edition of his work.*

* I regret the more that this mistake has occurred in Mr. Wood's "Treatise," because a writer in the "Dublin Review," taking advantage of it, has copied a page or two from Mr. Wood's work, in order to dispute the principles on which the proposed gradients are formed in the lines laid down by the Irish Railroad Commissioners. I may therefore, perhaps, be allowed to explain, that the Commissioners having reason to believe that the gradients, as laid down by the engineers, incurred an immense expense for earth work, &c., which the expected traffic would by no means justify, they endeavoured, by a circular issued to the engineers of working lines, to ascertain from practical experience the real amount of the impediments and disadvantages of different inclinations; and it was from a mean of these results that the tables and principles adopted in the Report were founded, and not on mere abstract scientific views, as represented by the writer in question. The table alluded to by Mr. Wood was derived from the same practical data, although it is not so stated in the work itself; the Report, at the time, not having been published.

In conclusion, I ought, perhaps, to apologise for laying these remarks, many of which may be said to be rather practical than theoretical, before a Society abounding in so much practical skill and information. Still, however, I am willing to hope that they may not be unacceptable.

If, as I have endeavoured to prove, the power expended is so much greater than the effect produced, particularly at high velocities, it is certainly very desirable, if possible, that this defect should be remedied; and if the resistance at high velocities *is* so much greater than at low velocities, and that this, in part, proceeds from the imperfections in laying the rails, or in other details in the construction, it will follow, that greater attention should be paid to this part of railway formation. And, lastly, if the mechanical impediments presented by inclined planes or gradients be comparatively less than has been hitherto supposed, greater economy than heretofore may perhaps, in future, be practised in the scale of gradients without any serious effect upon the working capabilities of the road.

Table (A)

Showing the Steam Power expended in Locomotive Engines in lbs. per Ton per Mile, with different Gross Loads, and at different Rates of Speed.

Name of Engines.	Dimensions of Engines.	Weight of Engine and Tender.	Weight of Load.	Gross Load.	Mean Maximum Speed per hour.	Mean Speed per hour.	Cubic feet of Water evaporated per hour.	Cubic feet of Steam per hour.	Bulk of Steam Water being 1.	Applied pressure in lbs. per ton of Gross Load.
		Tons.	Tons.	Tons.	Miles.	Miles.	Feet.	Feet.	Ratio.	lbs.
North Star.	Driving-wheel, 7 feet. Length of Stroke, 18 inches. Diameter of Cylinder, 16 inches.	28·89	15·9	44·79	41·15	38·51	197·7	77405	392	72
		28·74	32·92	61·66	36·97	31·52	163·8	63352	388	53
		29·01	41·61	70·62	38·8	33·9	136·5	68139	499	32
		28·68	53·58	79·26	30·63	25·3	153·6	50853	331	51
		28·57	82·03	110·61	32·81	30·66	200·9	61626	306	36
		28·41	166·36	194·7	23·3	18·63	141·4	37446	264	27
Harvey Combe.	Driving-wheel, 5 ft. Stroke, 18 inches. Diameter of Cylinder, 12 inches.	17·5	32·65	50·15	32·88	30·51	83·81	48288	576	29
		17·5	53·45	70·95	32·4	28·53	105·9	45103	425	32
		17·25	64·36	81·61	25·53	21·85	70·66	34579	489	22·7

Table (B)

Showing the Pressure per Inch exerted by Steam at different degrees of Density, or according to its Bulk, in comparison with Water at 60°.

Weight of Cubic foot of Steam.	Cubic feet of Steam to 1 of Water.	Pressure per square inch, in inches of Mercury.	Weight of Cubic foot of Steam.	Cubic feet of Steam to 1 of Water.	Pressure per square inch, in inches of Mercury.
Grains.		Inches.	Grains.		Inches.
254·7	1711	30	810	542	105
292	1497	35	910	479	120
363	1178	45	1110	391	150
427	1022	$52\frac{1}{2}$	1317	331	180
483	905	60	1520	288	210
593	737	75	1660	255	240
700	623	90	1910	229	270

Woolwich, 1839. PETER BARLOW.

V.—*Description of a Sawing Machine for Cutting Railway Bars.*

By JOSEPH GLYNN, F.R.S., M. Inst. C. E.

The advantages of railway bars being of equal and uniform length, and having their ends cut as nearly square as possible so that they may abut truly against each other, are so well known and appreciated that it would be needless to enlarge upon them. To ensure these advantages several attempts have been made, but none have been so successful as the mode of cutting off the ends by means of circular saws; this system has been adopted by the Butterley Company at their Iron-works in Derbyshire, and was first used there in the manufacture of the rails for the Midland Counties Railway. These rails are of the H form, as it is generally called, the upper and under surfaces of the rail being the same in shape, so that either may be used as the wheel-track; they nearly resemble the parallel rails used on the London and Birmingham line, but are much heavier, being 78 lbs. in weight to each single yard.

The circular saw has been extensively used for cutting iron, but to whom the merit of its original application is due is uncertain; the ordinary mode of using it was to have the ends of the rails as they came rough from the rolls, separately reheated, and alternately presented to the saw, measuring off by hand the required length. Under this arrangement much depended on the workmen employed; they were not always careful in presenting the bar at right angles to the plane of the saw, nor were they accurate in the adjustment of the length; the ends thus became distorted, and when laid, were out of line with the body of the rail.

To remedy these defects the sawing machine was constructed, and it has completely answered the purposes for which it was designed. The principle of the action of the saws is the well-known one of a disc of soft iron revolving with great rapidity, acquiring the power of cutting any hard substances applied to its periphery without wearing away even the file marks on its edge. The arrangement of the machine may be thus described.

The axis of the saws and the bed of the machine (which is exactly like that of a slide lathe) are placed at right angles with the line of the rolls, through which the rails are passed; the saws are fixed in lathe head-stocks, and slide

upon the bed so as to adjust them for cutting the rail exactly to the required length. The saws are 3 feet in diameter, and $\frac{1}{8}$th of an inch thick; they have teeth of the same size and shape as those of a circular saw for cutting wood, and they make 1000 revolutions per minute, so that the periphery of the saw moves at the rate of 9416 feet per minute, and consequently the teeth are in contact with the hot iron during so short a period that they receive no injury. In order, however, to avoid all risk, and to counteract the effects of the radiating heat of so large a mass of iron, the lower edge of the saw dips into water contained in a cup or recess cast in the head-stocks. The saw is secured between two discs of cast-iron faced with copper rings of such diameter that no more of the saw-plate is exposed than is necessary to cut through the rail.

The rail as it leaves the rolls weighs about 4 cwt., and is about 17 feet long; it is hastily straightened with wooden mallets upon a cast-iron plate, which receives it as it quits the finishing groove, and it lies right for sawing without any change of position, being parallel to the bed of the machine, and sufficiently hot for being cut. Thus the expense of reheating is avoided, and there is a saving of time and labour. The rest into which the rail is received is almost on a level with the ground, and is drawn forward by means of two racks; the pinions that work these racks are turned by a square axle passing through the centres of them, the axle being turned round by a hand-wheel fixed about the middle of it. Thus the rail is brought in contact with the two saws, and the ends are cut off by one operation. If the saws be sharp and the rail hot this is done in 12 seconds, if not, it requires 15 seconds to do it; the saw leaves a slight burr or fin at the section, which is removed by a file when the rails are cold. It should, perhaps, be stated, that when the rails are taken from the saws and are still hot, they are laid to cool in grooves planed in a thick cast-iron plate to fit their shape, so that they cannot warp or twist in cooling, but this is a refinement in rail-making which has no connection with the sawing machine, and may either be used with it or not.

The pivots or ends of the axles of the saws are of hardened steel in the form of a double cone, which prevents the oil from being driven out of the bearings when at a high speed, the centrifugal force keeping the oil at the joined base of the two cones. The bearings are supplied with oil by syphon cups or lubricators, such as are used for locomotive engines, and the machine is driven by belts, except where the motion is taken in the first instance from the line of

rolls, which is done by bevilled wheels; but the mode of communicating the first motion must depend on circumstances.

The Plate shows an elevation, plan, and end view of the machine (Figs. 1, 2, and 3); a section of the saw with the cast-iron discs faced with copper rings, and its axle, as before described, drawn to a larger scale (Fig. 4); a section of the rail and a portion of the saw, full size, showing the angle at which the teeth act upon the rail (Fig. 5); and two views (Figs. 6 and 7) of the sheet-iron hood which collects and confines the incandescent dust from the saw.

JOSEPH GLYNN.

VI.—*On the Expansion of Arches.*

By GEORGE RENNIE, F.R.S., &c.

THE expansion of solids has excited the attention of mathematicians and philosophers ever since the year 1688, when La Hire investigated the effects of heat and cold upon a rod of iron, down to the elaborate experiments of Mr. Daniell on the law of dilatation of metals between high and low temperatures, published in the "Philosophical Transactions" for the years 1830 and 1831. Between these periods we have the experiments of Ellicot, Smeaton, Roy, Ramsden, and Troughton, in our own country, and Muschenbrock, Borda, Lavoisier, Laplace, Dulong, and Petit, on the Continent. In general, however, their experiments have been directed to the law of the expansion of metals and glass, with reference to standards of measurements, but in no instance to their effects on buildings. Some experiments have been made in France, I believe, on the expansion of stones; and a very ingenious application of the expansion and contraction of iron rods in restoring the side walls of the gallery of the Conservatoire des Arts, at Paris, to their perpendicular position, was made by M. Molard.

Rondelet says, "Apprehensions have been entertained by some for the solidity of iron bridges, arising from the effects of dilatation and contraction by changes of temperature." But experience proves that these effects are no more to be feared in iron than in stone bridges, inasmuch as that the abutments being fixed, the arch has no alternative but to rise and fall according to the increase or diminution of temperature. If the arcs of the intrados and extrados of the voussoirs were of equal lengths, the arcs would move parallel with changes of temperature. But as the radii of the two arcs are different, an opening in the joints takes place and becomes an adequate compensation.

Vicat having observed that the arches of a stone bridge built by him over the Dordogne, at Souillac, were subject to periodical motions, concluded that it was owing to changes of temperature occasioned by the bridge having one of its sides exposed to the north and the other to the south, and concluded that all bridges were subject to the same alteration; but he does not seem to have investigated the elongations or contractions of arches independently of exposure.

In the year 1835, a paper by Mr. Adie on the expansion of different kinds of stones by increase of temperature, was published in the 13th vol. of the "Transactions of the Royal Society of Edinburgh." Mr. Adie states that his attention had been accidentally directed to the increase in the openings of a wall during a long-continued and severe frost which occurred in the year 1826; but it was not until the year 1830 that an interdict of the Dean of Guild, Court of Edinburgh, rendered the rate of expansion of stone a matter of more importance than merely a curious philosophical speculation. The reasons given for interdicting the operations were, "That the pillars had not sufficient strength to support the weight of the front, and that the difference of the expansion of cast-iron and stone was so great, that very prejudicial effects might arise from the use of such pillars in this situation." Mr. Jardine, of Edinburgh, was consulted on the part of the Dean of Guild, and Mr. Adie, sen., was requested by the proprietor of the building to calculate the difference of expansion between cast-iron and stone pillars. Sufficient data were easily got for the expansion of cast-iron, but, "the only experiments which could be found," says Mr. Adie, "are contained in a short notice by M. Destigney in the 7th vol. of the 'Quarterly Journal of Science, Literature, and Art.'" The experiment cannot be found in that Journal.

Mr. Adie further states, that since the commencement of his experiments he had "seen a notice in the 'London and Edinburgh Journal of Science' of a letter read on the 12th of March, 1834, to the Geological Society, by Mr. Charles Babbage, in which the author states, from the experiments of Colonel Totten recorded in 'Silliman's Journal,'* he has calculated a Table of the expansion in feet and decimal parts, of granite, marble, and sandstone, from which he finds the alteration in bulk so great, supposing the strata under the Temple of Jupiter Serapis to expand at the same rate as sandstone, and an increase of temperature equal to 100° to act on them to the depth of 5 miles, the temple would be raised 25 feet."

Mr. Adie then describes a steam pyrometer, and an ingenious mode of making his experiments on the expansion of various kinds of stones and prisms of cast-iron at 180° Fahrenheit, the results of which are given in a tabular form, and from which he concludes, "it is evident that no danger is to be apprehended from a change of temperature affecting cast-iron and sandstone in any great degree, as their expansion, so far as regards buildings, may be considered

* Colonel Totten's experiments on stone cannot be found.

as the same." Mr. Adie found the rates of expansion to be considerably influenced by the humid or dry states of the specimens, so that white marble particularly received a permanent increase of length with increased degrees of temperature.

There appeared to be no connection between the density of stones and their expansibility. In conclusion, Mr. Adie is of opinion that not the slightest degree of danger can arise from the use of cast-iron in buildings on account of the difference of their expansion for all ordinary degrees of temperature.

The introduction of iron in bridges, and the subsequent failure of several of these structures, over the Tame in Herefordshire, over the Tees at Yarm, and the iron bridge at Staines, for example, led to the conclusion that these failures were owing to the expansion and consequent pushing asunder of the abutments; and one of the arguments adduced against the iron arch proposed by Messrs. Telford and Douglas in place of the Old London Bridge, was the utter impracticability of constructing abutments capable of withstanding the expansion of so large an arch.

The same arguments were employed against the arches of the Southwark Bridge. In order, therefore, to satisfy public opinion, as well as his own mind, upon this subject, the late Mr. Rennie requested his son, the present Sir John Rennie, to undertake a series of experiments, upon the effect of temperature, on the arches of the Southwark Bridge. Accordingly preparations were commenced in the month of January, 1818, at the time when the main ribs and diagonal braces rested upon the centres, previous to the driving the iron wedges between the abutting plates and the stone-work, and before any of the spandrils and road-plates had been put upon them. The thermometer then ranged from 32° to 45° Fahrenheit. The first experiment was tried with iron and wooden wedges, both of which gave uniformly the same results.

First Experiments. The measurements were taken to $\frac{1}{40}$ of an inch, the results of which gave, for the first arch $\frac{7}{40}$, centre arch $\frac{10}{40}$, third arch $\frac{7}{40}$, for an increase of 13 degrees of temperature; or $\frac{1}{74}$ of an inch in first and third arch, and $\frac{1}{52}$ of an inch in centre arch, for 1° F. of increase of temperature.

Second Set. Experiments were also tried in the months of August and September of the same year, at a time when the arches were completely clear of the centres. Several others had previously been tried (by means of rods applied to the bearings of the centres, as well as by a spirit-level), though not sufficient to enable us to form any tolerable conclusion.

The total range of the thermometer, during the whole time, out of the sun, was from 42° to 75° = 33° F. variation.

The result of 10 experiments tried upon the 3 arches, with a range of the thermometer, from 54° to 75° = 21° F. variation.

For every 10 degrees of Fahrenheit—for the first arch $\frac{11}{40}$, for the centre $\frac{12}{40}$, for the third arch $\frac{11}{40}$ of an inch; or $\frac{1}{36}$ of an inch in the two exterior arches, and $\frac{1}{33}$ of an inch in the centre arch, for 1° F. in temperature.

The result of 23 experiments, on the third arch, gave $\frac{5}{20}$, or $\frac{10}{40}$ for 10° F. in temperature, or $\frac{1}{40}$ of an inch for each degree.

Mode of Observing. The mode of performing the experiments, consisted in having a stout piece of wood, 3 inches thick, properly planed and levelled, fixed to the top of the ribs of the centre, in three places: one on the east, middle, and west ribs, exactly in the centre of the arch, and about $\frac{3}{4}$ of an inch clear of the iron; fine planed wedges 3 inches wide, and adapted to the above spaces, were applied at different hours in the day, using two thermometers at the same time: one in the sun, and the other in the shade; and the rise and fall by these means were taken to about $\frac{1}{40}$ of an inch.

The eastern ribs seem to be more affected than the western; this of course is to be attributed to the longer continuance of the sun's rays upon the former than the latter; it is, however, upon the whole hardly worth while taking into the calculation.

Third Set. The following experiments, upon the Southwark arch, were tried by three accurate thermometers, by Dollond, graduated to half a degree: one hanging in the open air, another having the bulb immersed $1\frac{1}{2}$ inch in the iron itself, and the third hanging amongst the iron ribs, where the temperature upon the whole was much less liable to the sudden variations of the external atmosphere. To the former wooden block was attached an accurate brass scale, by Dollond, divided into $\frac{1}{80}$ of an inch, and a fine piece of feathered-edged brass nicely fixed to the rib, which, by the rise and fall of the arch, traversed upon the scale, and thus indicated the expansion and contraction with tolerable certainty.

I.

Thursday, September 17th. Commenced in the Morning.

Time.	Thermometer in Air.	Thermometer in Iron.	Thermometer among the Ribs.	State of Arch.
At 5 A.M. .	42°	Could not be seen.	42°	
6 .	,,	46°	45	Stationary.
7 .	,,	45	45	Rise just discernible.
8 .	,,	47	50	Risen $\frac{5}{80}$.
9 .	,,	48	51	Do. $\frac{1}{40}$.
10 .	,,	50	53	Do. $\frac{2}{40}$.
11 .	,,	52	55	Do. $\frac{2}{40}$.
12 .	,,	53	55	Do. $\frac{1}{40}$.
1 P.M. .	,,	54	56	Do. $\frac{1}{40}$.
2 .	,,	55	57	Do. $\frac{1}{40}$.
3 .	,,	55	57	Do. $\frac{1}{80}$.
4 .	,,	56	57	Stationary.
5 .	,,	56	57	Risen $\frac{1}{40}$.
6 .	,,	56	57	Do. $\frac{1}{80}$.
				Total rise for 15° F. $\frac{25}{80}$, or $\frac{5}{16}$ of an inch; that is, $\frac{1}{48}$th of an inch for 1° F.

II.

Friday, September 18th. Still, Foggy Morning—No Wind.

Time.	Thermometer in Air.	Thermometer in Iron.	Thermometer among the Ribs.	State of Arch.
At 5 A.M. .	48°	50°	49°	
6 .	50	50·5	50	Stationary.
7 .	50·5	50	50	Do.
8 .	51	51	51	$\frac{1}{80}$ rise.
9 .	52	51	52	$\frac{1}{80}$ do.
10 .	53	52	53	$\frac{1}{40}$ do.
11 .	56·5	53	56	$\frac{2}{40}$ do.
12 .	58	54	57	$\frac{1}{40}$ do.
1 P.M. .	58·5	55	57	0 do.
2 .	59	56	58	$\frac{1}{40}$ do.
3 .	60	56	58·5	$\frac{1}{40}$ do.
4 .	58	56	58·0	$\frac{1}{40}$ do.
5 .	57	56	58·0	Stationary.
6 .	57	55·5	57·5	Do.
¼ before 7 .	59	0	59	Falling just discernible.
				Total rise for 10° F. $\frac{16}{80}$, or $\frac{1}{5}$ of an inch; that is, $\frac{1}{50}$th of an inch for 1° F.

III.

SATURDAY, SEPTEMBER 19th. Very quiet, still Morning—No Wind.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 5 A.M. .	57°	58°	56°	Stationary.
6	56	58	56·5	Do.
7 .	56	58	57·5	Do.
8 .	59	59	58	$\frac{1}{80}$ rise.
9 .	62·5	61	59	$\frac{1}{40}$ do.
10 .	64	63	60	$\frac{2}{40}$ do.
11 .	64·5	63	61	$\frac{1}{40}$ do.
12 .	65	64	62	$\frac{1}{40}$ do.
1 P.M. .	64	64	62·5	$\frac{1}{80}$ do.
2 .	65	64	63·5	$\frac{1}{80}$ do.
3 .	65	64	63	Stationary.
4 .	65·5	64	63	Do.
5 .	63·5	63·5	62·5	Do.
6 .	62	63	62	$\frac{1}{40}$ falling.
				For $7\frac{1}{2}$° F. $\frac{13}{80}$ of an inch; that is, $\frac{1}{47}$th of an inch for 1° F.

IV.

WEDNESDAY, SEPTEMBER 23rd. Still Morning—A light Breeze.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 5 A.M. .	50°	51°	0°	
6 .	51	52	53	Stationary.
7 .	55	54	54	Just beginning to rise.
8 .	58	57	55·25	$\frac{1}{40}$ rise.
9 .	60	59	57	$\frac{5}{80}$ do.
10 .	60	60	58	$\frac{1}{40}$ do.
11 .	61	61	58·5	$\frac{1}{80}$ do.
12 .	64	63·5	60·5	$\frac{5}{80}$ do.
1 P.M. .	64	64	61	$\frac{1}{40}$ do.
2 .	65	65	63	$\frac{2}{40}$ do.
3 .	65	65	64	$\frac{1}{40}$ do.
4 .	65	64·5	64	Stationary.
5 .	63·5	64·5	64	Do.
				Total rise for 11° F. $\frac{22}{80}$ of an inch; that is, $\frac{1}{40}$th of an inch for 1° F.

V.

Thursday, September 24th. Mild, still Morning.

Time.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	State of Arch.
At 6 A.M. .	55°	55°	0°	
7 .	55	56	55	Stationary.
8 .	58	57	56	Do.
9 .	58	58	57	Do.
10 .	61	60	58	$\frac{1}{40}$ rise.
11 .	62	61	60	$\frac{2}{40}$ do.
12 .	66	63	60·5	$\frac{3}{80}$ do.
1 P.M. .	66	63	61	$\frac{1}{80}$ do.
2 .	65	63·5	62	$\frac{1}{40}$ do.
3 .	63	63	62	$\frac{1}{80}$ do.
4 .	61·5	62	62	Stationary.
5 .	61	61·5	61	Do.
6 .	59	60·5	61	Do.
				Total rise for 6° F., $\frac{13}{80}$ of an inch; that is, $\frac{1}{37}$th of an inch for 1° F.

VI.

Friday, September 25th. Mild, still Morning.

Time.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	State of Arch.
At 6 A.M. .	53	54°	0°	
7 .	54	55·5	55·5	Stationary.
8 .	54	56	56	Do.
9 .	58	57·5	57	Do.
10 .	58	57·5	57·5	Do.
11 .	58·5	58	58	$\frac{1}{80}$ rise.
12 .	60	59	58	$\frac{1}{80}$ do.
1 P.M. .	61	59	58·5	$\frac{1}{40}$ do.
2 .	64	61	60·5	$\frac{3}{80}$ do.
3 .	63	62	60	$\frac{1}{80}$ do.
4 .	62	62	60	$\frac{1}{40}$ do.
5 .	62·5	60·5	60	0 do.
6 .	61	60	59	Stationary.
				Total rise for 4·5° F., $\frac{10}{80}$ of an inch; that is, $\frac{1}{36}$th of an inch for 1° F.

VII.

SATURDAY, SEPTEMBER 26th. Morning Wet and Stormy.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 6 A.M. .	59°	57°	0°	
11 .	59	57	55	$\frac{1}{80}$ rise.
12 .	60	59	56	$\frac{2}{40}$ do.
1 P.M. .	62	60	56	0 do.
2 .	63	61	57	$\frac{3}{80}$ do.
3 .	63	60	58	$\frac{1}{80}$ do.
4 .	62	62·5	58	0 do.
5 .	61	60	58	0 do.
6 .	60	59	58	0 do.
became dark	..	..	..	Just falling.
				Total rise for 3° F. $\frac{9}{80}$ of an inch; that is, $\frac{1}{26}$th of an inch for 1° F.
				The outer Ribs were also observed, and gave the same result within a very small quantity.

VIII.

SUNDAY, SEPTEMBER 27th. A fine, mild Morning—the Wind southerly.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 7 A.M. .	54°	..	..	
2 P.M. .	62	..	..	$\frac{7}{40}$ rise.
				Total rise for 8° F. $\frac{7}{40}$ of an inch; that is, $\frac{1}{45}$th of an inch for 1° F.

IX.

MONDAY, SEPTEMBER 28th. Fine, mild Morning.

TIME.	Thermometer in Air.	Thermometer among the Ribs.	Thermometer in Iron.	STATE OF ARCH.
At 6 A.M. .	60°	60°	59·5°	
7 .	60·5	60·5	59	Stationary.
8 .	62	61	59	$\frac{1}{80}$ rise.
9 .	64	63	61	$\frac{2}{40}$ do.
10 .	66	65	64	0 do.
11 .	69	67	63	$\frac{2}{40}$ do.
12 .	72	69	64	$\frac{2}{40}$ do.
1 P.M. .	74	69	66	$\frac{2}{40}$ do.
2 .	73	70	67	$\frac{1}{40}$ do.
3 .	73	70	68	$\frac{1}{80}$ do.
4 .	69·5	68	67·5	$\frac{1}{80}$ do.
5 .	68	67·5	67	Stationary.
6 .	67·5	67	67	Do.
				Total rise for 7·5 F. $\frac{21}{80}$ of an inch; that is, $\frac{1}{28}$th of an inch for 1° F.

When these experiments were tried nearly all the road-plates were on, but none of the cornice, frieze, or parapet.

RESULT OF EXPERIMENTS.

The result of the preceding experiments being collected together, we have the following Table :—

No. of Experiment.	Total Variation in Temperature, in degrees F.	Total Rise in Arch, in parts of an inch.	Rise for 1° F.
I.	15°	$\frac{25}{80}$	$\frac{1}{48}$
II.	10	$\frac{16}{80}$	$\frac{1}{50}$
III.	7·5	$\frac{13}{80}$	$\frac{1}{47}$
IV.	11	$\frac{22}{80}$	$\frac{1}{40}$
V.	6	$\frac{13}{80}$	$\frac{1}{37}$
VI.	4·5	$\frac{10}{80}$	$\frac{1}{36}$
VII.	3	$\frac{9}{80}$	$\frac{1}{26}$
VIII.	8	$\frac{4}{80}$	$\frac{1}{45}$
IX.	7·5	$\frac{21}{80}$	$\frac{1}{28}$

The discrepancies in these results are not greater than is to be expected in observations of this nature. If we take the mean of the whole, the rise will be $\frac{2\cdot 1}{80}$, or $\frac{1}{40}$th of an inch, for each degree of temperature. Hence the total rise for 50° F. would be, according to the result of these observations, 1·25 inches.

CALCULATION FOR THE RISE OF THE ARCH OF SOUTHWARK BRIDGE ON AN INCREASE OF 50° F. IN TEMPERATURE.*

Length of chord of extrados . . . . 246 feet, or 2952 inches.
Versed sine, or rise of extrados . . . . 23 feet 1 inch, or 277 inches.

$$\text{Diameter of circle of extrados} = \frac{1476 \times 1476}{277} + 277 = 8141\cdot 89 \text{ inches.}$$

Then 4070·945 : 1476 :: radius : sin $\frac{1}{2}$ arch,

$$\begin{array}{r} 10\cdot 0000000 \\ 3\cdot 1690864 \\ \hline 13\cdot 1690864 \\ 3\cdot 6096904 \\ \hline 9\cdot 5593960 \\ \hline \end{array}$$

Whence $\frac{1}{2}$ arch $= 21° \ 31' \ 30''$,
And circumference of circle of arch = 3·1416 × 8141·89 = 25578·56 inches,
Therefore 360° : 21° 15′ 30″ :: 25578·56 : Length of $\frac{1}{2}$ arch (= 1510·4),
or length of arch = 3020·8 inches.

Taking the expansion of iron according to Lavoisier for 1° F. at ·00000618 its length, we have for 50° F.,

$$\begin{aligned} \text{the expansion of arch} &= 3020\cdot 8 \times 50 \times \cdot 00000618 \\ &= \cdot 9334272 \\ \therefore \text{ length of expanded arch} &= 3021\cdot 7334272 \end{aligned}$$

The height of the arch of which the chord is 2952 inches, and the length 3021·7334272, would be 278·9 inches.

* This calculation must be considered as an approximation only. It proceeds on the hypothesis that the arch, when expanded, will be a longer portion of the circle of the same radius. The curve will depend on the pressure which exists at every point, and consequently its exact form cannot be assigned.

Then $278 \cdot 9$ inches $- 277$ inches $= 1 \cdot 9$, or 2 inches nearly, which would be the rise of the arch consequent on 50° temperature.

Rise by calculation	1·9
Do. by observation	1·25
Difference between calculation and observation .	·75, or $\frac{3}{4}$ of an inch.

On comparing the mean of the experiments with the preceding calculation we find a considerable difference. The mean of the experiments on the rise of the centre arch gives a rise of $\frac{1}{4}$ of an inch for every 10 degrees of temperature; so that, taking the mean rise for every 10 degrees at $\frac{1}{4}$ of an inch, and the total range at 50°, we have a total rise of $1\frac{1}{4}$ inch for the centre arch, which is $\frac{3}{4}$ of an inch less rise than that given by preceding calculation. Considering, however, the frequent variations of temperature in our climate during the day, and the great mass of metal* exposed to its influence, the difference is not surprising; indeed it was found that the arches did not attain their maximum expansion or contraction until several hours after the maximum or minimum temperatures.

* The weight of the centre arch is 1665 tons, and of each of the side arches 1460; total, 4585 tons.

ON THE EXPANSION OF IRON UNDER THE ATMOSPHERIC VARIATIONS OF TEMPERATURE.

In the year 1818, being desirous of ascertaining the effect of temperature on the strength of iron, I suspended a bar of wrought iron, $\frac{1}{4}$ of an inch in thickness, in such a position that a lever attached to it should indicate, by the expansion of the iron rod, the variations of temperature on a graduated arc. The end of the lever was then loaded with a weight equivalent to within the limit of the average tension of a rod of similar dimensions, and the lever being placed exactly level, on a zero point, the variations of temperature were exhibited on either side of zero. The experiment commenced in October, from which period, as the weather became warmer or colder, the lever continued to rise and fall, morning, noon, and night, until a frost in November came on, when the bar was found broken. The conclusion was, that the iron had become weakened by the cold.

As a further example of the effect of expansion, but arising from a different cause, I may relate the following.

During the severe frost which took place in the winter of 1820, several of the lamp-posts, placed on the pedestals of the Southwark Bridge, were found broken at the tenons, or joggles, which fit into corresponding holes, or mortices, cast in the pedestals; several lamp-posts also were thrown out of their perpendicular position. On lifting off the lamp-posts, it was found that the holes were filled with ice, occasioned by the water lodging in them, and which, having been frozen, and consequently expanded, displaced the lamp-posts; and, in several instances, owing to the unequal depths of the holes, broke them at the tenons. To prevent a similar recurrence of these accidents, counter holes were drilled into the bottom of each hole laterally, by which the water now runs off, and nothing of the kind has since occurred.

A set of experiments was made at the request of Mr. Rennie, by Mr. Samuel Walker, of Rotherham, in the months of July and August, 1818, on the length of a large portion of the frieze-plates of the bridge at various temperatures. The frieze-plates, $2\frac{1}{2}$ inches thick, and 2 feet 10 inches high, were bolted together and raised upon a firm platform 3 feet from the ground. The length of the plates bolted together was 231 feet $\frac{1}{2}$ inch, at a temperature of 60° F. The temperature of the atmosphere and of the plates was noted at 5 o'clock in the morning and 3 o'clock in the afternoon, the coolest and hottest portions of the day; thermometers for this purpose being suspended in the air and laid on the plates. The results are given in the following Table.

TABLE showing the EXPANSION and CONTRACTION of the FRIEZE PLATES of SOUTHWARK BRIDGE.

JULY, 1818.	Therm. 5 A.M.	Therm. 3 P.M. in the Shade.	Therm. 3 P.M. in the open Air.	Therm. 5 A.M. laid on the Plates.	Therm. 3 P.M. laid on the Plates.	Length of the Plates, 5 A.M.		Length of the Plates, 3 P.M.		REMARKS.
						Feet.	In.	Feet.	In.	
17th	60°	76°	73°	62°	83°	231	1	231	$1\frac{5}{8}$	Weather very sultry—rain just begun—thermometer exposed to the rain.
18th	60	76	84	59	93	231	$0\frac{1}{2}$	231	$1\frac{1}{2}$	Weather at 3 P.M.—hot, but rather cloudy.
20th	70	70	$71\frac{1}{2}$	68	78	231	$0\frac{3}{4}$	231	$1\frac{7}{32}$	Weather at 3 P.M.—cloudy.
21st	63	74	79	60	84	231	$0\frac{1}{2}$	231	$1\frac{1}{8}$	Rather cloudy—occasional sun.
22nd	63	75	79	63	87	231	$0\frac{1}{2}$	231	$1\frac{3}{16}$	Sunshine, and rather windy.
23rd	68	81	96	68	99	231	$0\frac{3}{4}$	231	$1\frac{9}{16}$	Clear sky, and rather hot.
24th	68	87	98	68	104	231	$0\frac{3}{4}$	231	$1\frac{7}{8}$	Clear sky, and brisk wind—very hot.
25th	64	82	96	65	95*	231	$0\frac{7}{8}$	231	$1\frac{15}{16}$	The day had been bright till 3 P.M., when it suddenly became windy, and rather cloudy or hazy.
27th	64	67	61	63	62	231	$0\frac{5}{8}$	231	$0\frac{11}{16}$	Cloudy, wet, and no sun.
28th	53	68	73	50	85	231	$0\frac{1}{8}$	231	$1\frac{1}{4}$	Sun shone, but cloudy.
29th	61	81	82	61	89	231	$0\frac{7}{16}$	231	1	Brisk wind, a little sun, and rather cloudy.
30th	66	$73\frac{3}{4}$	79	65	82	231	$0\frac{1}{2}$	231	$0\frac{7}{8}$	A little wind, and cloudy.

* The Thermometer, when laid on the plate, and concealed from the sun, did not vary. Being applied underneath the plate, it stood at 92°.

AUGUST, 1818.	Therm. at 5 o'clock.	Therm. at 2 o'clock, in the Shade.	Therm. at 2 o'clock, in the Sun.	Therm. at 7 o'clock.	Therm. laid on Plates at 5 o'clock.	Therm. on Plates at 2 o'clock.	Therm. on Plates at 7 o'clock.	Length of 17 Plates at 5 o'clock.		Length of 17 Plates at 2 o'clock.		Length of 17 Plates at 7 o'clock.		REMARKS.
								Feet.	In.	Feet.	In.	Feet.	In.	
5th	60°	79°	108°	84°	58°	113°	89°	231	$0\frac{9}{16}$	231	$1\frac{10}{16}$	231	$1\frac{5}{8}$	Very hot, and a clear sky.
6th	66	$71\frac{1}{2}$	74	66	67	81	65	231	$0\frac{5}{8}$	231	$1\frac{3}{8}$	231	$1\frac{1}{8}$	Cloudy, and a brisk wind.
7th	53	65	77	66	51	81	65	231	$0\frac{1}{4}$	231	$1\frac{7}{16}$	231	$1\frac{3}{16}$	Clear sun, and a cool brisk wind.
8th	58	64	67	64	55	68	64	231	$0\frac{3}{8}$	231	$0\frac{13}{16}$	231	$0\frac{9}{16}$	Cloudy, and a little wet.

ON THE EXPANSION OF STONE BRIDGES.

In the year 1834, two years after the completion of the stone bridge over the Thames, at Staines, and after the arches had attained their full settlement, openings were observed in the joints of the parapets immediately over the springing of the arches, and a distortion, or sinking of the upper curve of the parapets, was the result. Convinced of the true cause, I inserted into each of the openings an iron wedge until it descended to an inch and a half, and with a tracer marked the lowest point of descent of each wedge. This was in the month of January, when the weather was very cold. The same wedges were carefully inserted in the openings, once a week, until the month of May, when the wedges could no longer enter, as the joints had closed so firmly, as, in some instances, to chip off small pieces of stone from the upper edges of the parapet. The reverse effect, however, was found to take place with the joints immediately over the crowns of the arches, which, from being quite close before, were then open. The conclusion was, that the arches had been affected by variations of temperature: contracting, and, consequently, descending in the winter, and opening the spandril joints, and expanding and closing these joints in the summer; thus the joints of the parapets (which were made of single slabs of granite, for the whole height) became good indicators of the changes of temperature. These phenomena had often been noticed by me in the Waterloo and other bridges, where, from the joints having been made good with Roman cement in the winter, were found broken, or rather crushed, in the summer. No doubt therefore remained of the fact, although it was desirable to verify it by experiment. Accordingly, I procured several samples of granite, sandstone, and slate, as nearly as possible of similar dimensions, and, having placed them in a properly constructed oven, ascertained their rates of expansion, by scales and micrometer screws. The specimens were measured as they received a fresh increase of heat, and, when taken out of the oven, were placed in a wooden box, and allowed to cool gradually, so that the decrease of heat might be measured in the same manner. The results will be found in the annexed Table.

From the preceding statements, we may conclude that all buildings are subject to expansion; but that the range of temperature, arising from solar heat, cannot be prejudicial to their stability.

EXPERIMENTS ON THE EXPANSION OF IRON AND STONE.

By GEORGE RENNIE, Esq.

March and April, 1834.

MATERIAL.	Length in Feet.	Breadth; parts of a Foot.	Thickness; parts of a Foot.	Contents; parts of a Foot.	Temperature, F., in Air.	Temperature, F., when Heated.	Expansion in Total Length; Feet.	Rate; the Length in Air being 1.
Cast Iron . . .	4·886	·416	·175	·356	32°	70°	Not perceptible	..
Do.	..	..	..	..	..	86	·003472	1·0007104
Do.	..	..	..	..	..	200	·008333	1·0017049
Do.	..	..	..	..	..	100	·003472	1·0007104
Do.	..	..	..	..	..	150	·0052083	1·001065
Granite	5·	·5	·3	·833	45	220	·01458	1·002916
Do.	..	..	..	..	..	100	·00208	1·000416
Clay Slate . . .	5·00625	·75	·2083	·791	46	70	..	..
Do.	..	..	..	..	..	87	·00208	1·000416
Do.	..	..	..	..	..	104	·00347	1·000693
York Paving . .	4·916	·416	·25	·512	46	95	·00833	1·001695
Micacious Sandstone	..	..	..	..	..	98	·00333	1·000678
Do.	5·166	·5	·25	·646	46	70	..	..
Do.	..	..	..	..	..	98	·00694	1·001344
Do.	6·	·6	·25	1·	52	200	·010416	1·001736
Do.	..	..	..	..	45	180	·00625	1·001041
Do.	..	..	..	..	52	200	·00625	1·001041
Do.	..	..	..	..	..	150	·00416	1·000832
Do.	..	..	..	..	..	100	·003125	1·000520
Do.	5·	·5	·25	·625	52	122	·00416	1·000832
Do.	..	..	..	..	..	140	·00333	1·000666
Do.	..	..	..	..	45	100	·002083	1·000416
Do.	..	..	..	..	..	160	·00625	1·00125
Do.	5·	·416	·25	·521	45	260	·00729	1·001458
Stock Brick . .	..	..	..	..	52	260	..	1·0025

CALCULATIONS OF THE EFFECT OF 50° INCREASE OF TEMPERATURE ON THE MIDDLE ARCH OF STAINES BRIDGE.

	Feet.	In.
Chord of extrados	82	3, or 987 inches.
Versed sine	9	4, or 112 inches.

$$\text{The diameter} = \frac{493 \cdot 5 \times 493 \cdot 5}{112} + 112 = 2886 \cdot 48 \text{ inches,}$$

$$\text{and radius} = \frac{2286 \cdot 48}{2} = 1143 \cdot 24$$

$$\text{Then}\quad 1143{\cdot}24 : 493{\cdot}5 :: \text{rad} : \sin \tfrac{1}{2}\text{ arc}$$

$$\begin{array}{r} 10{\cdot}000000 \\ 2{\cdot}693287 \\ \hline 12{\cdot}693287 \\ 3{\cdot}058137 \\ \hline 9{\cdot}635150 \\ \hline \end{array}$$

and $\frac{1}{2}$ arch $= 25° \; 34' \; 409''$.

The semicircumference $= 1143{\cdot}24 \times 3{\cdot}1416 = 3591{\cdot}602784$

$$180° : 25° \; 34'{\cdot}409 :: 3591{\cdot}6 : \text{length of } \tfrac{1}{2} \text{ arc}$$

$$10800 : \quad 1534{\cdot}4 \quad :: 3591{\cdot}6 : 510{\cdot}27.$$

Length of whole arch $= 2 \times 510{\cdot}27 = 1020{\cdot}54$

Amount of expansion $= 1020{\cdot}54 \times {\cdot}00000438 \times 50° = 0{\cdot}22349826$

Length of expanded arch $= 1020{\cdot}54 + 0{\cdot}22349826 = 1020{\cdot}76$ inches, &c.

The height or versed sine of an arc, of which the chord is 987 inches, and the length $1020{\cdot}76$ inches, would be $112{\cdot}3$ inches.

$112{\cdot}3 - 112 = 0{\cdot}3$, which is the rise consequent on 50° increase of temperature.

From the extremity of chord to top of parapet is 176 inches, and $493{\cdot}5 : 176 :: 0{\cdot}3 : 0{\cdot}10$ inch, which is the effect of this rise in a lateral direction at the level of the parapet over the centre of each pier.

$$\text{Chord of } \tfrac{1}{2} \text{ arch} = \sqrt[2]{(112)^2 + (493{\cdot}5)^2} = 506{\cdot}0 \text{ inches.}$$

$$\text{Chord of expanded } \tfrac{1}{2} \text{ arch} = \sqrt[2]{(506{\cdot}0)^2 - (112{\cdot}3)^2} = 493{\cdot}38 \text{ inches.}$$

The lateral movement of one spandril, from the crown of the arch, by the rise,

$$= 493{\cdot}5 - 493{\cdot}38 = 0{\cdot}12 \text{ inches.}$$

The whole lateral movement from the crown of the arch by the rise, each spandril receding the same distance from the middle point,

$$= 2 \times 0{\cdot}12 = 0{\cdot}24.$$

In addition to the above lateral movement, there would be a departure of each spandril from the perpendicular to the crown of the arch, equivalent to

what would be produced by a movement round the point at the crown by a fall of $0 \cdot 3$ at the distance of $\frac{1}{2}$ the chord of the arc. Height from crown to parapet-level is 66 inches.

Then $493 \cdot 5 : 66 :: 0 \cdot 3 : 0 \cdot 04$. The departure of one spandril from the perpendicular.

The whole separation of spandrils at parapet-level over the crown of arch due to this revolving motion,

$$= 2 \times 0 \cdot 04 = 0 \cdot 08.$$

Total opening at the level of parapet over the crown,

$$= 0 \cdot 24 \times 0 \cdot 08 = 0 \cdot 32.$$

The expansion of *one* spandril (that is, between centre of arch and centre of pier) would be $= 493 \cdot 5 \times 50° \times .00000438 = 0 \cdot 108$ inches.

This expansion would tend to diminish the opening already mentioned as taking place above the crown, and would tend to increase the compression which takes place above the piers.*

The following are the results of the experiments of M. Destigny on the expansion of stone and metals.

ON THE EXPANSION OF STONE, AND THE MEANS OF MEASURING IT. By M. DESTIGNY.†

The following were the results at 40° Reaumur, or 122° Fahrenheit :—

The expansion of Iron being	196
That of clear white Carrara Marble, second quality.	136
That of French Marble of Sost	91·10
That of Marble of St. Béat	67
That of St. Leu Stone	104
That of Stone of Vernon-sur-Seine	68·95

* The multiplier ·00000438 for the expansion of granite, is taken from the experiments on the expansion of stone by Alexander J. Adie, recorded in the "Transactions of the Royal Society of Edinburgh," Vol. XIII.

† See "Bulletin de la Societé d'Encouragement;" also, see Vol. X. of "Repertory of Arts," page 376.

TABLE of the absolute EXPANSION of the different STONEY SUBSTANCES, as well as of BRASS and IRON, for a variation of Temperature of 100° Centigrade, or 212° Reaumur.

SUBSTANCES.	Absolute Expansion.	Expansion for the Length of one Metre.
Brass	0·00187821	1·8782
Soft Wrought Iron . .	0·00122045	1·2200
Carrara Marble . . .	0·00084867	0·8487
St. Béat Marble . . .	0·00041810	0·4181
Sost Marble	0·00056849	0·5685
Vernon Stone	0·00043027	0·4303
St. Leu Stone	0·00064890	0·6490

M. Destigny found no difference in the expansion of the stone, whether dry or moist.

VII.—*Description of the State of the Suspension Bridge at Montrose, after it had been rendered impassable by the Hurricane of the* 11*th of October,* 1838; *with Remarks on the Construction of that and other Suspension Bridges, in reference to the action of violent Gales of Wind.*

By C. W. PASLEY, C.B., Colonel Royal Engineers, F.R.S., Hon. M. Inst. C. E., &c.

In the afternoon and night of the 11th of October, 1838, I travelled from Perth to Inverness by the mail, in extremely boisterous weather, which I afterwards learned had nearly approached to a hurricane, having torn up a great number of trees, swept away the produce of whole fields of corn, and otherwise caused extensive damage to property over the whole of the North of Scotland. From Inverness I went to Aberdeen, whence it was my original intention to proceed direct to Dundee; but having heard that the same storm had destroyed part of the suspension bridge of Montrose, I was induced to stop there, that I might have an opportunity of inspecting the construction of that bridge, and of ascertaining from what cause, or, at least, in what part, it had given way; having always been of opinion that from the example of failures some of the most instructive lessons in practical architecture or engineering are to be derived. I found on inspection that about one-third part of the roadway of the bridge had been entirely carried away, excepting a sort of external skirting-board or ornamental fascia, which remained in a shattered state on the west side of the bridge, hanging by the suspension rods, which were not entirely broken, but only injured and bent on that side; the chains, however, four in number, which extended in two parallel lines of two tiers each, remained apparently quite perfect. The length of roadway between the two piers of masonry which supported the chains appeared to be about 410 feet, and over this space the links of each chain consisted of six iron bars, each 1 inch thick and 5 inches deep, and the connecting plates of seven pieces of the same width but shorter and deeper in the usual proportion.

I was informed at Montrose that the whole of the chains were originally constructed with four bars only, but that in consequence of one of them giving way soon after the bridge was finished, two more bars had been added to each, in the central part only. The cause of this accident was said to have been a boat race, at which a multitude of spectators suddenly rushing from one side

of the bridge to the other broke one of the upper chains, and thereby caused the loss of some lives. The failure having proved the insufficiency of the chains in their existing state, two plans were proposed for increasing their strength, the one by adding a third chain similar to the former over each side of the bridge so as to make six chains in all, in two tiers of three each; the other plan was to increase the strength of the existing chains by adding two more iron bars or plates to all the parts of each, and connecting them by new bolts longer in proportion. This plan (suggested by Mr. Rendel) was adopted and executed under his direction in the central part of the bridge, but not at the extreme ends of it, where it was unnecessary, as the roadway did not depend upon them, being there formed of solid masonry. The efficiency of this judicious arrangement has since been sufficiently proved by the circumstance of all the four chains remaining uninjured during the partial destruction of the roadway.

The guard of the mail coach and other persons who passed over the bridge just before it gave way, stated that they felt the bridge vibrate in the most violent manner, and that they had always experienced the same sort of vibration in a greater or lesser degree during every strong gale of wind. In Mr. Provis's account of the Menai bridge, he states that no transverse vibration of the roadway ever took place during the most violent storms, but that the whole of it undulated longitudinally like the waves of the sea, which is corroborated by my own observation of the action of a strong gale of wind upon a light suspension bridge for foot passengers over the Tweed at Melrose; in which, though about 300 feet long and only six feet wide, not the smallest transverse vibration or lateral movement was perceptible, although the wind blew broadside on, but a considerable undulation was going on continually throughout its whole length. I therefore consider, that the persons who described the suspension bridge at Montrose as having been in a state of vibration, must either have confounded that term with undulation, which is sometimes done, or erroneously ascribed its unsteadiness when they passed over it, to a motion of the former instead of the latter sort.

On my return to Chatham by way of London, I inspected the suspension bridge at Hammersmith on the 30th of October, two days after a very violent storm that had done a great deal of injury in that town and neighbourhood, and blown down two walls and part of the roof of a new building then in progress in the brewery of Sir F. Booth at Brentford. About a month afterwards, on a very strong gale of wind setting in, I went to inspect the same bridge a second

time, when I found no undulation going on; and having made inquiry of persons who were in the habit of passing over it in all weathers, I ascertained that this bridge has never been affected in that alarming manner.

When I compared the construction of the roadway of this bridge with that of Montrose, I became convinced of the correctness of an opinion I had previously formed, that all injuries that have ever occurred to the roadways of suspension bridges must have been caused by the violent action of the wind from below, such as I had an opportunity of observing at Chatham, when looking down upon the Royal Dock Yard at that place in a tremendous hurricane, on the 29th of November, 1836, when the wind acting from below on the far side of one of the roofs of a large ship-building shed, caused it to rise into view from time to time, for when it was in its proper position, I could not see it at all. Thus it continued for more than an hour flapping up and down, and adhering to the other side of the same roof by the ridge only, in the same manner that a person may move the cover of a book with his hand; until at last I saw a large fragment of it carried away, which, as I afterwards ascertained, measured about 40 feet by 50, and which, after floating a little while in the air, as a sheet of paper would in a light wind, fell to the ground at the distance of 50 yards, breaking through part of the roof of a storehouse. If the planking and purlins of this roof had been bolted down to the principal rafters, and secured from below, this accident would not have happened. On that same day, the hurricane overset a stage coach on Rochester bridge, and blew away part of the stone balustrade of that bridge; and also destroyed part of the roadway of the suspension chain pier at Brighton, and I have no doubt of its having done so by acting upon it from below. That this is the usual action of the wind in hurricanes was sufficiently proved by the fate of the roof in Chatham Dock Yard; and supposing it to act thus on a suspension bridge, it must first force the roadway of the bridge upwards, until its own weight, or a temporary lull of the violence of the wind, may cause it to fall down again to the full length of the suspension rods, in continual undulations, the rods being thereby alternately subject to compression and extension must give way at last, under the same process which a person naturally adopts when he wishes to break a piece of wire or light iron; and in the course of this process, the chains also being at one time relieved from part of the weight of the roadway, and afterwards having it thrown upon them by sudden jerks, must be put in motion. Such, I repeat, must have been the sort of action, by which a consider-

able portion of the roadway of the chain pier at Brighton, and also of the suspension bridge at Montrose, were destroyed at the periods above mentioned.

The reason of the superior safety of the suspension bridge at Hammersmith became evident to me, on comparing the construction of the roadway of that bridge with those of Brighton and Montrose. In the latter there were only light railings on each side of the bridge, composed chiefly of slight iron bars, which, in the chain pier at Brighton, were all vertical; but in that of Montrose they intersected each other diagonally; but in neither of those sort of parapets was there such stiffness in a vertical direction, as to prevent the undulation of the roadway in a violent gale of wind or hurricane, although they might check it in a slight degree during a moderate breeze. It was, however, observed in the case of the chain pier at Brighton, the partial destruction of which was witnessed by numerous spectators, that no part of the roadway gave way in the hurricane of November, 1836, until the side railings were previously shattered to pieces and blown away, after which the undulations became perceptibly more violent, and ended in carrying away the roadway also. In the suspension bridge at Hammersmith, on the contrary, there are four lines of strong trussing along the whole length of the roadway from one end of it to the other, so firmly connected by iron bolts and plates to the roadway bearers below, which consist of double joists of wood, that it is quite impossible for the strongest gale of wind to produce any undulations in the roadway, without breaking all the parts of these trusses to pieces, which are far too strong to be thus acted upon, being composed partly of cast-iron, and partly of substantial wood-work. The side railing of the Plate. Fig. 3. Hammersmith bridge is formed by hollow cast-iron pillars about $4\frac{1}{2}$ feet high, at intervals of about 15 feet apart, having a longitudinal sill below and a cap or top rail above, with two braces intersecting each other diagonally in the form of the letter X, all of these last-mentioned parts being of wood-work, and of considerable scantling. In addition to these there are two other lines of trussing, consisting of a series of king-posts, each formed by a hollow cast-iron pillar, similar to those before described, and a couple of wooden braces in the form of rafters, these pillars being at intervals of about 25 feet apart, the whole resting on a strong longitudinal sill, and each pair of small rafters abutting at top against the heads of those cast-iron pillars, and at bottom against cast-iron blocks. These lines of king-post trussing being immediately under two of the four lines of chains, by which the roadway is supported, so as to divide the carriage-way, which is in the centre of the bridge, from the footpaths on each

side of it, are reduced in height in the middle length of the bridge, where those two lines of chains come down close to the roadway. The two other lines of chains are over the sides of the bridge, and the vertical rods suspended from them pass close to the trussed side railings before described; and thus, as there are two tiers of chains in each line, there are eight chains in all, the bars of which are all 1 inch in width by 5 inches in depth; but while there are six bars in each link of the two centre lines of chains, there are only four bars in the others, which support the outer sides of the foot paths.

To illustrate these particulars, I have annexed a sketch of the suspension bridge of Montrose in elevation, as it appeared when I inspected it five days after it had been rendered impassable (*see* Fig. 1). In this sketch I have omitted the useless fragment of skirting-board or fascia before mentioned, on one side of the bridge, which would have rendered the figure confused. I did not pay much attention to the precise construction of the side railing at that time, and therefore the sketch of it given in Fig. 2 is from memory; but it was certainly in no respect stronger than that shown in the sketch. I have also annexed sketches in elevation of the side railing and king-post trusses of the suspension bridge of Hammersmith (*see* Figs. 3 and 4).

On an attentive perusal of Mr. Provis's account of the Menai bridge, I am sorry to say that it does not appear to me to be so secure, against the effects which may be apprehended, from the violent undulations to which it has always been subject, as could be desired. By this observation I mean no disparagement to my honoured friend, that most excellent man and distinguished engineer, the late President of this Institution, by whom the Menai bridge was designed, and under whose directions it was executed, because engineers had then no experience of a suspension bridge of so very large a span; and in constructions entirely and absolutely new, no man, however great his talents may be, can be expected to foresee every contingency that ought to be provided for. I believe that Captain Brown's suspension bridge over the Tweed, at Norham, near Berwick, said to be about 360 feet in length between the points of support, was not only the largest, but the most ingenious and judicious bridge of this sort that had ever been constructed, until Mr. Telford, by authority of the Government, undertook the gigantic task of constructing a bridge over the Menai Strait, at such a height as to admit the largest sailing vessels to pass without striking their top-masts, by the successful execution of which, he not only exceeded all former suspension bridges in magnitude, but

made, as I conceive, a great improvement, by adopting rectangular iron bars 1 inch wide, and more than 3 inches deep, for the links of his chains, and giving them altogether a much more perfect form than those of Captain Brown, who used round iron, both for the parts of his chains and for his suspension rods, not only in his bridge over the Tweed, but also in his chain pier at Brighton. Before the Menai bridge was quite finished, a very violent gale of wind broke some of the suspension rods, and also a few of the roadway bearers which were of wrought iron; and as the resident engineers reported to Mr. Telford, it caused not only a very great and continual undulation of the roadway, but made the chains, 16 in number, which were arranged in four lines of four tiers of chains each, to vibrate, and even slightly altered the position of one of them. It appears, however, that the persons who witnessed these movements, must have considered the lateral motion of the chains as the most important, and as being the cause of the undulations in the roadway, for immediate precautions were taken to counteract the former, by introducing two parallel rows of cast-iron tubes about four inches in diameter, transversely, which connected and stiffened all the chains of the upper tier, and all those of the lower tier but one, and were themselves connected vertically by a sort of net-work, of light wrought-iron rods, intersecting each other diagonally, as represented in Fig. 6. Mr. Provis states that after this addition, the undulations of the roadway in violent storms of wind were reduced to about one-half of their former height. But we know that they have not been entirely prevented, for I have been informed by several gentlemen, who have inquired minutely into the state of the Menai bridge during strong gales of wind, that the undulations are then always very violent. Mr. Rickman, junior, has stated that he was informed by the toll-keeper, that the difference of level in very violent gales was not less than 6 feet, from the roadway rising and falling 3 feet above, and as much below, its proper and usual level. From this circumstance, contrasted with the stability of the Hammersmith bridge, in all weathers, we may now infer, although it was not foreseen at the time, that it would have been better to truss the roadway of the Menai bridge, on the principle afterwards adopted by Mr. W. Tierney Clark, the engineer of the Hammersmith bridge, than to stiffen the chains by the iron tubes and diagonal lacing before described. In fact, the roadway, from its very great surface, presents such an immense obstruction to the fury of a hurricane acting upon it from below, that if it be prevented from undulating, it can scarcely be supposed

that the utmost force of the wind could move the chains at all, having comparatively so very little surface to oppose to it, and which must be held down by the great weight of the roadway, so long as *that* remains at rest. Let it be understood, that I do not consider it any discredit to the very able resident engineers and skilful workmen employed at the Menai bridge, when the first storm occurred, in stating my opinion, that they made a mistake in recommending Mr. Telford to stiffen the chains, instead of the roadway, because they had not the experience which we possess in the present day.

After I had inspected the suspension bridge at Hammersmith, I called upon Mr. Clark, who informed me that before he decided definitively upon all the details of the construction of that bridge, he made a model of it on a very large scale, which he exposed to all weathers, and which must have given him more insight into the action of the wind on a suspension bridge, than theory or reflection alone could have done; besides which, he had the experience of the undulations of the Menai bridge, as a result to be guarded against. Mr. Clark showed me his designs for several new suspension bridges, one of which is to be thrown over the River St. John's in New Brunswick, in which he proposes to use a simpler mode of trussing than that adopted by him for the bridge at Hammersmith, which I consider an improvement. I also called upon Mr. Rendel, on learning that he was again employed to direct the repairs of the bridge of Montrose, in consequence of the late hurricane, and was pleased to find that his opinion, as to the destructive action of the wind on the roadway from below, agreed with mine; and indeed he assured me that, in an official letter written previously to this event, he had recorded his opinion, that he considered the roadway to be the weakest part of that structure. Mr. Rendel was also good enough to show me a design for trussing the roadway of a new suspension bridge, different from Mr. Clark's, but which could not fail to be extremely efficient. As both these gentlemen are members of this Institution, I shall not presume to anticipate any thing further that they may be pleased to communicate; but as my ideas on the peculiar action of violent gales of wind upon suspension bridges were derived from my own observation, before I communicated with either of them, and as the mode of preventing it adopted by Mr. Clark in the suspension bridge at Hammersmith, has been proved to be effectual, it gives me pleasure in having an opportunity of ascribing to that engineer, the merit which he deserves, of having been the first to make this important improvement in the construction of suspension bridges.

The only printed paper in which I have seen the necessity of a stiff trussed railing for the sides of a suspension bridge alluded to, is one by my brother officer, Lieut.-Colonel Reid of the Royal Engineers, recently appointed Governor of the Bermudas, and the author of a very important Essay on the Law of Storms, who after having observed the destructive effects of a tremendous hurricane that took place at Barbadoes, immediately before his arrival there, which naturally drew his attention to this subject, was afterwards an eye-witness of the destruction of part of the roadway of the chain pier at Brighton, in November 1836; and as the very same part of that pier had been destroyed on a former occasion at night, by a violent storm of wind, accompanied by thunder and lightning, he states his opinion, that the wind also had been the cause of this previous failure, and not the lightning, as had generally been supposed, and he remarks on the iron rods of the side railing of the chain pier being all arranged vertically without any diagonal bracing, which he considers a defect; but the experience of the Menai bridge, in which the side railing is composed of a complete net-work of light iron, not less than 7 feet high, as represented in Fig. 6, sufficiently proves that no diagonal bracing of this sort can prevent those violent undulations, to which bridges not trussed on Mr. Clark's principle must always be liable.

That the Menai bridge, notwithstanding its great length between the points of support, has not yet been seriously injured, I ascribe to the peculiar system adopted by Mr. Telford, of making his suspension rods in several pieces with joints, which converts each of them, or at least all those composed of three or more pieces, into a sort of chains, thus giving them that sort of flexibility, which prevents their being alternately subject to such very violent compression and tension, as those of the bridge of Montrose, of which all the rods in the centre part, where the roadway was carried away by the fury of the hurricane, were entirely broken on one side, and those on the other side opposite to the fracture, generally bent, and probably also partially broken. The light wrought iron roadway bearers of the Menai bridge were also evidently less liable to be injured by those undulations, than inflexible bearers of cast-iron, such as those of the Montrose bridge would have been. But though the Menai bridge has hitherto escaped with no material injury, until the late tremendous hurricane of the 6th and 7th of this month, which was so destructive at Liverpool, and which has injured it, as I am told, very slightly, still it appears desirable that this magnificent national work should be secured for the future, by at least two

if not four lines of inflexible trussing, in all the central part of the roadway, between the two great towers of masonry which support the chains, and which for symmetry, though not from necessity, might be continued along the remaining parts of the roadway also.

I am informed that the particular portion of the roadway of the chain pier at Brighton, which has twice given way, has subsequently to its second failure been trussed by a vertical segment of wood over the central part of it, with chains led down from each end of this segment, and fixed to the two adjacent clumps of piles by which the towers that carry the chains are supported. But there being four inverted arches formed by the chains of the suspension pier at Brighton, unless the other three portions of the roadway be trussed also, I have no doubt but that some future hurricane will produce the same injurious effect upon one of these, though they have hitherto escaped.

As the injurious action of violent winds upon suspension bridges, if I am right in my views concerning it, must depend on the length and width of the bridge; and as hurricanes, though much less frequent, and generally less violent than in tropical climates, have occurred, and must again be expected in this country from time to time, I am of opinion that the roadway of every suspension bridge having only light side railings, such as those at Montrose, Aberdeen, &c., and indeed of all that I have seen, excepting that of Hammersmith, will sooner or later be carried away by some future hurricane, provided that it shall approach to, or exceed 400 feet in length between the points of support. The necessity of at least two inflexible lines of trussing for the roadway of such bridges has hitherto been so little attended to, that I thought it might be useful, at least to the junior members of this Institution, to bring it pointedly under their consideration.

Chatham, Jan. 29, 1839. C. W. PASLEY, Colonel R. Engineers.

NOTE.—Since the preceding paper was read I have been informed by Mr. H. R. Palmer, one of the Vice-Presidents of the Institution, who assisted Mr. Telford when the original plans for the Menai Bridge were under discussion, and went through the whole of the details with him, that the necessity of trussing the roadway was the subject of much anxious consideration; that Mr. Telford foresaw the probability of trussing being required, but finally decided upon omitting it in the first instance, and adopting it subsequently should experience prove it to be necessary.

C. W. P.

VIII.—*On the Supply of Water from Artesian Wells in the London Basin, with an Account of the Sinking of the Well at the Reservoir of the New River Company, in the Hampstead Road.*

By ROBERT W. MYLNE.

The sinking of Artesian Wells is a subject in which the inhabitants of London have taken considerable interest during the last few years, and public Companies have been projected for the purpose of supplying a portion of the Metropolis with pure and soft water by that means.

The construction of Artesian Wells is now generally understood, many having been sunk in England, as well as on the Continent, where they obtained their name, from having been originally adopted in the French province of Artois, called by the Romans Artesium. These wells are made by boring vertically through a deep stratum of clay into one of sand, where water is generally found, and from whence it will rise to a considerable height, in many instances to the ground surface, and sometimes higher. This greatly depends upon the elevation from whence the sand stratum is supplied at the verge of the basin, within which the stratum of clay is situated.

The strata under, and around this Metropolis (designated as the London Basin) are peculiarly adapted for these wells, the whole being comprised in an immense bowl of chalk, many miles in extent, on the interior surface of which there is a thick lining of sand supporting a deep bed of clay, commonly known as the London Blue Clay; and upon this the Metropolis, and adjacent country, stands.

The sand which lies below this clay has always been found considerably charged with water, derived, no doubt, from the surface of the more distant country, and entering where the sand makes its appearance on the surface, which may be termed entering at the verge of the basin. This outcrop, or margin of sand, in some instances, is at a great height above the level of the Metropolis. It therefore necessarily follows, that, on boring into the basin, the water will rise in the bore-holes to various distances from the surface, according to the elevation of their respective situations.

Under these circumstances, most of the wells already sunk in and about London obtain their supply of water; and from this source it has been asserted that a very large quantity of water might be procured for the various uses of the inhabitants, while others consider it so very precarious, and the water so difficult to obtain, that they prefer boring down into the chalk, where they suppose an inexhaustible supply can be found.

With a view to obtain as much information as possible on this point, a well has been sunk by the New River Company: a brief account of which will be found in this paper, setting forth all the material circumstances which occurred during the execution of the work. But before entering on the detailed account of this particular well, it seems desirable to mention a few circumstances connected with other wells, which have come under the knowledge of the writer, and which, perhaps, may be considered to throw some light on the subject.

First, as regards those wells which are supplied from the deep *sand springs* below the bed of clay. There is one at St. Alban's House, Piccadilly, where the water rises to within about 50 feet from the surface of the ground; also another at the banking-house of Messrs. Coutts and Co., in the Strand, which formerly rose above the surface of the ground, and discharged itself into the Thames; but, from other wells having since been sunk in the neighbourhood, the water surface has lowered several feet, and at present is below the low-water level of the river. At Norland House, Kensington, there was one which ran over for many years; and when the spring was first struck, it was so strong that it washed the well-sinkers out at its mouth; this great overflow continued some time, but has now ceased from the effect of other wells sunk in the vicinity.

Instances have occurred around London of the water rising to great heights: at Tottenham there were several wells that ran over; and also at Tooting, Mitcham, and Hanwell; but all of these have more or less diminished in their discharge, and but few rise to within many feet of the surface of the ground which they formerly overflowed.

In the town of Cambridge, which stands upon clay, a considerable number of wells have been sunk, most of which rose above the surface; but they are now much reduced, both in height and quantity.

At Gloucester the depth of the wells vary more than in other places, although the town is situated on ground gently falling towards the river;

instances exist here where persons have obtained a good supply of water at a reasonable depth, while attempts to obtain a supply at a considerable depth, by their immediate neighbours, have utterly failed, after incurring great expenses.

Of those sunk or bored into the chalk, in the vicinity of London, the most remarkable as regards the strength of the springs, and the quality of the water, are those around Wandsworth. One in particular, at some Copper Works: where, from a large bore-hole, the water is forced out to the height of about 20 feet above the ground. For convenience it has been confined by a bent tube, so as to deliver the water horizontally into the mill-head. Several others occur along the banks of the River Wandle, all delivering above the ground surface, and so plentifully that they give considerable power to the mills on that river.

The proprietors of the larger wells, have generally found that there is an intimate connection between all that derive their supply of water from the sand stratum.

A striking instance of this is afforded by the well in Messrs. Calverts' brewery, in Thames Street, which is materially affected when Messrs. Barclay and Perkins work theirs in Southwark, although situated on the opposite side of the River Thames. Also the supply at Messrs. Whitbread's brewery, in Chiswell Street, which is one of the earliest wells of any magnitude, was so much inconvenienced from this cause, that they were compelled to bore to a very great depth, and thus obtain a more permanent supply through vertical pipes.

Messrs. Combe and Delafield's supply, in Castle Street, Long Acre, was also affected in this manner, and on deepening the well it completely destroyed another that had been sunk for the establishment of a saw-mill in St. Martin's Lane.

At one period, in sinking the well at the Hampstead Road reservoir (before alluded to), the water was likewise found to be sensibly affected to the extent of from 10 to 14 feet in height, by the working of an engine at the well at the Zoological Gardens, in the Regent's Park, which terminates in the sand.

It is needless to enumerate the many further instances that might be adduced to establish this fact; I shall, therefore, now advert to the difficulties that present themselves in obtaining water from the sand springs, when subjected to a considerable draught.

The sand is of so fine a nature that it will pass through the smallest apertures,

and is easily put in motion, from its specific gravity being little more than that of water, and, from its extreme sharpness, it is found to cut the pump barrels exceedingly; large quantities of sand will in this way be drawn up with the water from beneath the clay, thus forming large cavities below, and causing the clay to sink, by which the foundations of all buildings situated near the well are endangered.

A very remarkable instance of the subsidence of the ground occurred at the Hampstead Road well, where the quantity of sand raised by the engine through the 8-inch pump was such as to cause a very serious settlement in the large raised reservoir adjoining, by separating the high banks into two distinct portions; damaging a culvert, and snapping a line of iron pipes asunder. This no doubt would have affected the adjoining houses, had not the pumping been discontinued. A similar case happened at Messrs. Reid and Co.'s brewery, in Liquorpond Street, where the well, after the engine was set to work, during the time of sinking it, was found to have created such a cavity below, that the proprietors were obliged to close it almost entirely, to save their buildings from ruin. At the Vinegar works in the City Road, the well, from the same cause, was altogether abandoned for manufacturing purposes; as also a large well at the brewery of Messrs. Ramsbottom and Co., in Broad Street. At Whitechapel there was another well, belonging to Major Rhode, where it was found, on inspection, that the withdrawal of the sand, by pumping, had formed an immense cavity underneath the plastic clay; this caused a material subsidence of the ground, and 20 feet of the lower part of the brick shaft disengaged itself, and falling to the bottom, the fragments were completely buried in the quicksand.

Many other instances might be mentioned of wells having been abandoned, from the quantity of sand raised, and occasioning great loss of property, through the sinking of the surface-ground; but it will be needless to pursue the subject any further.

It therefore appears, that little dependence can be placed upon the quantity to be derived from the *sand spring*, and also that a great risk attends the obtaining it in large quantities. To obviate these difficulties, it has lately been considered advisable to sink through the sand into the *chalk*, which has been done generally by the means of boring, and the introduction of a small pipe; but as yet there is little experimental knowledge on the subject; and, from the few instances that have occurred in the Metropolis, a sufficient number

of facts have not been collected to enable a correct opinion to be formed, as to the quantity of water that can be obtained from such a source.

Among the larger wells that have been sunk into the chalk, there has been one lately executed at Brighton, for the supply of that town with water, which produces an abundance of fresh water from below the level of the sea. The water here issues from the fissures in the chalk, which are intersected by horizontal headings driven from the bottom of the well, from whence the water is pumped up for the supply of the town.

A singular circumstance happened in cutting through the chalk hills, for the formation of a tunnel for the Thames and Medway canal: that operation had the effect of draining the whole of the fresh water from the wells within the range of a mile, and substituting salt water in those wells.

A large bore was also made in the chalk valley through which the River Lea passes in Hertfordshire, where the layers of flints in the upper part of the chalk yielded a small quantity of water; but no increase took place beyond the depth of 100 feet, although the well was continued to the depth of 300 feet, at which point the chalk marl began to make its appearance; but the tool having broken in the bore, the work was abandoned.

In Paris, where a basin of clay exists similar to that of London, a bore-hole has been in operation for a long period, and is at present being carried on at the depth of 1360 feet in the chalk, and 1490 feet below the surface of the ground; as yet no quantity of water has been obtained.

It will therefore appear, from the few instances which have been mentioned, that different situations will be affected by varying causes; and from the little information that can be collected, with regard to sinking wells in the chalk, no rule can at present be laid down upon which an opinion can be formed as to the possible results of an experiment at any particular place.

Having offered these preliminary observations on the subject of wells, I take leave to subjoin a section of the well lately sunk at the New River Company's reservoir, in the Hampstead Road; together with some sketches of the tools used during the works, and a statement of the mode in which the work proceeded. The execution of this work was placed under the immediate care and superintendence of Messrs. Hunter and English, experienced millwrights, whose attention was unremitting during the operation.

PARTICULARS OF THE SINKING THE WELL AT THE HAMPSTEAD ROAD RESERVOIR, THE PROPERTY OF THE NEW RIVER COMPANY.

Wm. C. MYLNE, ENGINEER.

On the 21st March 1835, the well-sinkers commenced excavating in made ground, to the depth of 6 feet, and continued through 17 feet of red (Hampstead) gravel, at a diameter of 20 feet; the sides of the shaft were supported by strong wooden curbs of a less diameter than the excavation, and clay puddle was filled in behind them in order to shut out the land springs. These curbs were sunk down through the bed of gravel, and made to enter a few feet in the London blue clay lying immediately below it. At this depth a cast-iron footing was fixed within the wooden curbs, upon which a 14-inch brick shaft was carried up to the ground surface, of a clear diameter of 12 feet 6 inches, worked in Roman cement, and the cavity of 6 inches, that remained between the back of the brickwork and wooden curbs, was carefully filled up with concrete.

The excavation was continued through the stratum of blue clay at a reduced diameter of 12 feet 6 inches, and steined with 9-inch brickwork in cement; thus leaving a clear shaft of 11 feet diameter.

The brickwork was built by continual underpinnings, as the work proceeded; and cast-iron rings were inserted at about every 8 feet in depth, projecting beyond the back of the brickwork a few inches into the clay, for the purpose of supporting the shaft as it progressed.

On attaining the depth of 57 feet in the clay, the boring auger, that was always kept about 6 feet in advance in the centre of the well, gave indications of having passed through the blue clay into a stratum of soft mottled clay; it was therefore thought advisable to discontinue the brickwork, in order to ensure a foundation for its support.

On proceeding with the excavation, the diameter was again reduced to 10 feet 9 inches, for the purpose of introducing, as a substitute for brickwork, cast-iron cylinders, formed of 6 segments, 6 feet in length, united by bolts through flanches projecting on the inside, leaving 9 feet 9 inches clear diameter; these were joined together, and being forced down by hand-screws, were made to follow the work as the sinking continued in the remainder of the blue clay,

and then through a 10-feet bed of soft mottled clay: on approaching the bottom of this stratum, water first made its appearance.

An engine and two 8-inch pumps, in two lifts, were erected and set to work, by which the well was kept dry. The sinking was thus continued with the same sized cylinders, following the work to the further depth of 8 feet, in a bed of fine brown sand: during the progress through this sand the quantity of water considerably increased, occasionally accompanied by large quantities of sand blowing up above the lower edge of the cylinder; this was observed chiefly to come from the north-east; and on boring a hole in the cylinder at that quarter, in the mottled clay stratum, it was found that the clay had fallen down, leaving a cavity extending 3 feet beyond the cylinders. From the very unequal pressure, occasioned by this cavity, the cylinders were forced out of the perpendicular, and became completely jambed when their lower edge was within a foot of the bottom of the sand stratum.

From this circumstance it was found necessary to commence with cylinders of a sufficiently reduced diameter, so as to be placed within the others, to allow them to sink perpendicularly.

During the period that elapsed in preparing these smaller cylinders the water rose to within 85 feet of the surface of the ground, and the fine brown sand within the cylinder to nearly the level of its original surface.

On this second set of cylinders being ready, the water was pumped out of the well, and the sinking proceeded with through the accumulated sand and the remainder of the sand stratum, and entered another bed of mottled clay, similar to the former. There was then found a gut of sand on the north-east side, upon the surface of the clay, to the depth of 2 feet; extending 4 feet on the circumference of the well, and diminishing towards the centre.

During the sinking of the cylinders through this bed of mottled clay, the water and sand entered in large quantities from the same side as the gut, to the great inconvenience of the workmen; and on reaching the bottom of the stratum, which was 19 feet 6 inches in depth, they came upon a thin layer of pebbles, so closely embedded in black sand as to form a complete mass of stone, which was broken through with difficulty by hammers and chisels: the layer being from 9 to 12 inches in thickness, and the pebbles averaging about the size of an egg: on leaving this, a bed of dark brown sand presented itself, and was sunk into, to the depth of 4 feet 6 inches, from which there was little

increase of water beyond that still continuing to run from the north-east side. This run of water was found, on inspection, to have occasioned a similar cavity behind the cylinders in the last bed of clay, as that before described in the upper bed, with the same attending consequences, the cylinders being forced out of the perpendicular, and again jambed in the sand; when, after several ineffectual attempts to force them down by powerful hand-screws, it was found necessary to commence with a third set of cylinders, of a further reduced diameter of 7 feet 4 inches.

The sinking then proceeded through the remaining 7 feet of dark brown sand, and into a bed of quicksand, of a darker colour, to the depth of 5 feet; where, on the 19th January 1836, the cylinders became so jambed that it was again found impossible to sink them lower.

During the latter portion of this work great difficulty was experienced from the frequent blowing-up of the sand, often to the height of 6 feet at a time: and also from the immense quantity of sand that was pumped up with the water, continually choking the pumps. This had caused not only the large cavities already mentioned behind the cylinders, but the lower part of the brick shaft was observed to have inclined bodily towards the north-east; thus causing numerous serious cracks, and in some places so affecting the shaft that it became of an oval figure. Several segments of the cast-iron cylinders were also broken asunder at their vertical flanches. This led to an examination behind the brickwork. An opening was made in the brick shaft, about 60 feet from the surface, where a cavity was found, extending many feet at the back of it; this was immediately filled up with stones and brickbats, concreted with lime and gravel. Several lengths of cast-iron cylinders were then placed on the top of the second set, for the purpose of lining the brick shaft; and all the irregular spaces between them and the brickwork were filled up with good concrete.

This settlement of the ground not only extended itself to the works in immediate connection with the well, but also to the reservoir banks contiguous, which were found to have sunk so much, that a large portion of the embankment and inside lining of brickwork, on the side nearest the well, was separated from the remainder, and the cast-iron main beneath the bank was broken in several places.

From the circumstance of these extensive settlements, it was considered advisable to discontinue the pumping, and thus suspend the works for a short

time, in order to consider of some other mode of getting through the remainder of the sand stratum; of the many plans suggested one was adopted, by the recommendation of Mr. Simpson (engineer to the Chelsea water-works), whose Report is annexed. It is there proposed, that the sinking should be continued with the water remaining in the well.

In accordance with this recommendation, on the 22nd August 1836, the 8-inch pumps were taken out, and a wrought-iron cylinder, or tube, formed of boiler-plates, rivetted together to the length of 62 feet, with a clear diameter of 5 feet 10 inches, was lowered to the surface of the sand, which had risen 7 feet in the well since the discontinuance of the works; the level of the water, which varied considerably at times, generally stood about 102 feet from the ground, thus leaving a column of water of 29 feet in the well.

Across the top of the tube, which stood 33 feet above the water, a platform was placed for the workmen to carry on their operations.

The sand now having the pressure of water upon it, was found to have become hard, requiring to be loosened by chisels. After that, the sand was excavated by an instrument called a miser: the different misers employed varied in size and figure, but were all constructed on the same principle, and each capable of holding about 2 bushels of sand, although they seldom came up more than half full.

In this manner the sinking was continued, while the wrought-iron cylinder was forced down by hand-screws that were placed on its upper edge. After thus forcing through the remainder of the quicksand stratum, a bed of sand, intermixed with small flints and pebbles, was sunk into, to the depth of 7 feet; on passing through which a bed of chalk was entered, containing numerous small flints, very closely embedded, to the depth of 4 feet 6 inches. This stratum was excessively hard and difficult to penetrate, thus rendering the progress of the works exceedingly slow: at the bottom of this stratum the chalk became much more solid, and the cylinder was sunk into it, to the depth of 12 inches; at which period the works were discontinued for a short time, leaving the well at a depth of 151 feet from the surface of the ground.

During the sinking through these different strata the wrought-iron cylinder had been lowered on an average from 4 to 5 inches per day, and the works kept constantly in operation.

It was now considered that the water and sand would be entirely shut out, and that the sinking might be continued in the usual manner. The platform

was then cleared away, and two 8-inch pumps, in two lifts, were put up within the wrought-iron cylinder, and on its being set to work by the engine the well was freed from water.

Towards the end of March 1837, the workmen commenced excavating in the hard chalk, at a diameter of 6 feet. The chalk was now found so solid as to require no lining; and, as the works proceeded, layers of flints were cut through, running about 2 feet 6 inches apart, and from 9 to 12 inches in thickness: each of these layers yielded water from their under-side, and they continued at such intervals to the depth of 33 feet, or 183 feet from the surface of the ground.

At this depth, one of the layers being partly broken into, produced more water than heretofore; and from this extra quantity (added to that yielded from those above) the 8-inch pump was found incapable of keeping the water lower than within 7 feet of the bottom.

The sinking was therefore discontinued, and attention was directed to the better securing and finishing the works above. The chalk was excavated to an enlarged diameter below the bottom of the wrought-iron cylinder, for the purpose of forming a brick footing, of a diameter of 4 feet 7 inches in the clear, increasing at the bottom to 7 feet 9 inches, being 10 feet 6 inches in length; on the top of this brickwork a broad cast-iron ring, in segments, was fixed, projecting a few inches beyond the back of the brickwork into the chalk, upon which rested the cast-iron cylinders that were introduced within the wrought-iron tube for the purpose of strengthening it, and to guard against the admission of sand, in case of its failure from corrosion.

These cylinders are of a clear diameter of 4 feet 7 inches, and were erected one over another to the height of 35 feet; and the 31 feet of wrought-iron tube that remained standing above them was cut off, leaving 31 feet of it in the well.

It was now considered advisable, in order to obtain as much water as possible, to admit such quantity from the sand-spring as was practicable without the admission of sand; to effect this, three $1\frac{1}{2}$-inch cocks were inserted in the cylinders nearly at the top of the dark brown sand stratum, and the water flowed through them mixed with a small quantity of sand.

The whole of the works having now been properly secured and made complete, about the end of February 1838 the 8-inch pumps were taken out and two of 12 inches diameter introduced, in two lifts. During a considerable

time that elapsed in preparing for the erection of these pumps, the water surface in the well had remained at a level of 108 feet from the surface of the ground. In the month of August 1838, being the period at which the springs are short, and having also been preceded by a dry winter and summer, the engine was set to work, and the result of two weeks' experiment, as to the quantity of water raised, is shown below. Another experiment was also made in March 1839, being the period at which the springs usually produce their utmost, and following a rainy winter season, the result of which is also annexed.

Engine set to work 1st August 1838.—Surface of water in the well 109 feet from the ground.—Depth of water in the well 74 feet.—One 12-inch column of water, in 2 lifts.

	Cubic Feet.
Quantity of water raised during the first week . .	103,950
Ditto ditto second week .	104,625
	2)208,575
Average cubic feet raised per week	104,287

Or 14,898 cubic feet per day of 24 hours.

Engine set to work 20th March 1839.—Surface of water in the well 105 feet from the ground.—Depth of water in the well 78 feet.—One 12-inch column of water, in 2 lifts.

	Cubic Feet.
Quantity of water raised during the first day . . .	30,584
Ditto ditto second day . .	30,415
	2)60,999
Average cubic feet raised per day of 24 hours . .	30,499

SUMMARY of the Expenses incurred in sinking the well, erecting an engine and pumps, and making good all damages occasioned during the execution of the work:—

	£.	s.	d.
Excavations, cartage, labour, &c.	3,006	2	0
Steam-engine and machinery	1,912	17	10
Millwrights'-work, well-sinkers, castings, &c.	5,762	8	6
Bricklayers' and masons' work	1,740	5	9
	£12,421	14	1

The following table shows the depth of the different strata of earth, through which the well was sunk:—

	Feet.	In.
Made ground	6	0
Red gravel	17	0
London blue clay	59	0
Soft mottled clay	10	0
Fine brown sand	9	0
Soft mottled clay	19	6
Black sand and pebbles	1	0
Dark brown sand	11	6
Dark quicksand	6	0
Sand, with flints and pebbles	7	0
Chalk, with small flints	4	0
	150	0
Chalk, with flints, as far as has been excavated .	33	0
Total depth	183	0

Trinity high-water mark, 77 feet 8 inches below the surface of the ground.

REPORT OF J. SIMPSON, Esq., ENGINEER.

SIR, Thames Bank, Pimlico, March 30th, 1836.

Having, in pursuance of your request, surveyed the well and works at the New River Company's Hampstead Road reservoir, and obtained the necessary information, I have now the pleasure to submit the following Report.

With respect to the stability of the well and works, I have to observe, there is a settlement in the second brick curb, about 29 feet from the surface; towards the bottom of this curb it is very much distorted, and there is every indication of its having been forced over towards the reservoir by the pressure of the earth.

The well-diggers have had to contend with much more than the ordinary obstacles generally encountered in such undertakings; the subsidence of the soil immediately contiguous to the curb and cylinders has been great, and notwithstanding the precautions adopted of sustaining them and the pumps by long iron bolts, serious injury has accrued to the curb and cylinders, and the well should not, in my opinion, be proceeded with until they are effectually secured.

It has been stated to me, that, during the sinking of the lower cylinder, the sand was continually forced under it into the well, whenever the spring got vent, more especially on the side next the reservoir; and there are sufficient indications on the surface to show that the subsidence of the earth has been very extensive—there is no doubt but that the settlements in the reservoir have been caused by it; and from the appearance of the walls of the cottage, the subsidence has also proceeded in that direction; and, although difficult to ascertain its precise limits, it seems to me we may conclude that it ranges from one to two hundred feet round the well; the quantity of sand dug out from the bottom appears greatly to have exceeded the cube of the well at the depth of the lower sand stratum: from the state of the water I saw pumped out on the 17th inst., it contained from $\frac{1}{8}$ to $\frac{1}{12}$ sand and clay, the colour of the latter being frequently discernible; and from the occasional gushes of clay, sand, and water, through holes which had been bored in the lower cylinder, to prevent its flowing over the top, on to the well-sinkers, it is manifest there is a great subsidence of the soil round the curb now going on, and that it proceeds most rapidly when the water is pumped out of the well.

From examples of wells bored and sunk in and near the Metropolis, particularly on the western side, viz., Hammersmith, Fulham, &c. &c., the springs in the chalk formation are evidently more abundant, and in most instances rise much higher than the springs from the sand stratum above it; nearly all the overflowing wells near London derive their supply from the chalk formation; the water in most cases is found at about 70 feet depth in it, sometimes in strata of loose flints, sometimes from fissures, and occasionally from soft veins in the chalk. Sufficient proof of this has been obtained by borings in and near the Metropolis, and by connecting the observations with the cases of wells actually sunk into the chalk immediately outside the London basin; the fact of the water rising to higher levels may be attributable to the more open structure of the formation compared with the sand strata, but it is more probably attributable to the sources from whence the supplies are derived being in more elevated situations; and there seems already sufficient evidence to connect a considerable portion, if not the whole, of the sources of supply with the rivers and streams traversing the bassetts of the strata, where they crop out and intermingle with the chalk on the uplands near the Metropolis.

The chalk formation is evidently the stratum from which large supplies of water can reasonably be anticipated to effect the object; however, so far as my practice has directed me, a shaft must be sunk, and adits driven, to open and unite the fissures in what may be really termed the cavernous structure of the chalk, to admit of the water flowing to the well as freely as possible. The difficulty and expense of sinking a shaft through the sand strata in or near London has hitherto been a serious obstacle: boring has been the expedient resorted to; and although, by this process, large supplies of water have been obtained, I am of opinion the real capabilities of the chalk formation, as a water-bearing stratum in this part of the country, have not yet been fully developed.

To return to the object more immediately in view, viz., the sinking of the well in the Hampstead Road safely into the chalk, I have to state, that the work has occupied my most serious attention, previously to the adoption of any plan. I am of opinion, it will be *advisable to secure the present brick curbs and upper cylinders* with strong timber segments and strutts; and wherever derangements of the curbs and cylinders are discernable, to fit in and substantially fix strong scantling vertically between them and the segments, to counteract the lateral thrusts now acting against the curbs and cylinders

from the pressure of earth on the opposite sides to the great vacuities in the soil contiguous to the well.

The surrounding soil will, no doubt, be saturated with water to the utmost extent until July; and I recommend further proceedings to be delayed until the commencement of that month, when it may be reasonably anticipated that the soil will become more consolidated, and be in a state more suitable for the prosecution of the undertaking.

With regard to the recommencement of the work, only two of the plans which have occurred to me are deserving attention.

1st. The driving of an iron pile curb.

2nd. The sinking of iron cylinders cast in entire circles.

The model of the iron pile curb you shewed me appeared very well designed for the purpose in view; the driving it, however, would be a work most difficult to execute.

According to the section of the strata before mentioned, which, in the absence of better information, I must quote, the bottom of the third cylinder may be taken to be from 13 to 14 feet above the chalk; and, as it is not practicable to keep the sand lower in the cylinder than 6 feet above the bottom of it, the drift of the curb, with 3 feet in the chalk, will be from 22 to 23 feet. I do not see how such an extent of drift can be effected with less than three tier of pile curbs, especially as the water must be kept pumped out of the well for the performance of the work, which will throw considerable pressure against the piles; and the quantity of sand which will inevitably be forced under the cylinders with the water, will prove a serious interruption, independent of causing the further subsidence of soil round the well, added to which the concussions arising from the blows of the ram of a pile-engine at such a depth will affect the mass to a serious, if not to a dangerous extent. I cannot, therefore, recommend this mode of proceeding.

The sinking of iron cylinders through sand and gravel, without pumping out any of the water within the shaft, is now a very common practice among the well-diggers; it is performed with the common boring rods and tools, the shells, or buckets, are fitted with valves at the bottom to open upwards; they are much larger than those used in borings, and the men turn and force them into the strata and draw the material up in them with the greatest ease; when the cylinders become set, they make use of a small sling ram, or occasionally of a very heavy sledge-hammer, to jar them, and they resort to this expedient

whenever they find the cylinders do not sink according to the proportions of sand or gravel they remove. The keeping of the water in equilibrium inside and outside the cylinders is very important; I have practised this method, and from my observation of its efficacy in several instances, I recommend its adoption at the well in the Hampstead Road; it may involve the necessity of two sets of cylinders, but I think, if the first be made strong enough a second will not be required. The top of the cylinder is always kept above water, which will be an advantage in the case in question; it will enable the men to work in a part of the well where there is much more room, and afford them greater facilities for their operations.

In case my observations have not extended to any point, or to every particular, you directed my attention to, I request the favour of you to inform me, that I may supply the deficiency.

I remain, Sir,

Your most obedient servant,

(Signed) JAMES SIMPSON.

Wm. C. Mylne, Esq., Civil Engineer,
New River Head.

IX.—*Account of the Gravesend Pier.*

By WILLIAM TIERNEY CLARK, F.R.S., M. Inst. C. E.

Previous to the introduction of steam packets on the River Thames, Gravesend was a place of resort for many persons from London in the summer months; the passage by the sailing packets, or tilt boats, as they were anciently called, presented an agreeable but uncertain conveyance. This transit was never attempted except with the tide, and frequently the passengers, being becalmed, were compelled to land at some place short of the vessel's destination, or to remain on board until the following tide; and it was not uncommon to be detained on the water throughout the night, especially when the journey commenced after mid-day. These sailing packets were of a burthen varying from 15 to 35 tons, and were capable of carrying from 60 to 100 passengers. A coach, called a tide coach, awaited the arrival and departure of the packets, for the convenience of those who resided at Rochester, or other places in the immediate vicinity of Gravesend. At the period referred to (1820) the population of Gravesend and the adjoining place, called Milton, was under 5000; and the visitors were chiefly of the lower classes of society. These packets either drew too much water, or it was considered inexpedient to allow them to lie alongside the jetty; and, consequently, in order to embark or disembark, the passenger had to hire a small boat or wherry to convey him between the shore and the packet, for which each person paid sixpence, although the distance would not exceed from ten to twenty yards, and although the same wherry would convey eight persons. The passage between Gravesend and London generally occupied from five to six hours, because, though a fair and fresh wind might have carried the vessel the whole distance in three hours, these favourable circumstances were invariably made the cause of delay in starting, in order to get a larger number of passengers, or, in the words of those days, "a good tide;" the whole object of the Captain (who was also generally part owner) being to reach the terminus by the time the tide was exhausted. When a steam vessel first came to Gravesend the attempt was made to lay her alongside the jetty, so that the passengers

might walk in and out; but this attack upon vested rights was so stoutly resisted by certain parties, that the owners of the packet soon made an unconditional surrender; and the watermen, seeing that their easy earnings had been placed in jeopardy, reduced their fare between the shore and packet to fourpence each person. The trip to and from Gravesend, however, being rendered certain by the steam packet, the resort to the place gradually increased, and at length, in the year 1830, in consequence of continued remonstrances by the visitors against being compelled to use the wherry boats, and a general demand for a pier, a public meeting of the inhabitants was called by the Mayor, to consider the propriety of erecting one. In the notice for convening the meeting, however, the Mayor thought it expedient to disclaim giving any judgment of his own, and to throw out a suggestion that, provided the pier should be built, compensation should be made to the watermen. The meeting was fully attended; but most of the persons who signed the requisition either were absent, or became at the moment opposed to the erection of a pier, and the measure was consequently condemned; for not more than four or five inhabitants were bold enough to declare their real sentiments, among whom was the then Town-clerk, who, for his temerity, was dragged from the Town Hall, and assaulted by the mob.

Notwithstanding this discouragement, a meeting of proprietors of the steam-packet companies was convened early in 1831, to express their opinion upon the utility of a pier; and although individual interest very much encouraged the hope of success, intimidation and prejudice prevailed to such an extent, that the steam-packet proprietors declared that a pier would be injurious to their establishment.

Public opinion, however, pressing closely, the municipal authorities of Gravesend, in 1832, applied to the legislature for authority to erect a pier. Perhaps a greater parliamentary contest on a subject of local improvement never occurred; nor were stronger feelings and interests ever called forth. The promoters of the Bill had opposed to them many of the inhabitants, as well as the watermen; who, with the mariners generally, declared the proposed plan to be impracticable, on account of the obstruction to the navigation, and as tending to impede the supplies for the London coal and fish-markets. The leading argument of the opponents of the plan was, that, as it was proposed to carry out the new pier to a considerable extent beyond the existing quay and jetty, the flow of the tide would be obstructed thereby in a much greater

degree than by the old jetty with its numerous arches. This was, however, met on the other side, by proof that there was a much greater extent of opposing surface in the old jetty than in the proposed pier, although the latter was to extend 40 feet beyond the lowest point of the jetty. Some of the authorities, also, connected with the conservancy of the Thames, took active measures of opposition; and, finally, at the close of the session, after powerful struggles and divisions in every stage, and an expenditure of a large sum of money, the Bill was lost in the House of Lords by a majority of one.

The struggle in Parliament, however, had called up the notice of the public, and even of the Ministry: and in consequence of some particular features in the report of the Lords' Committee, the Duke of Richmond, then one of the Cabinet, prevailed on their Lordships to recommit the Bill. But, after a second expensive struggle in the Committee, it met with a similar fate, and was consequently lost.

The contest, however, was not without its advantages, for, in the course of the investigation, the authorities of Gravesend became not only confirmed in the propriety of their conduct, but received so much public encouragement, that they determined, during the recess, to erect a temporary pier, and to shew to Parliament, in the ensuing session, not only the practicability of the proposed pier without injury to the navigation, but also the local as well as public benefit which would follow such a measure.

Accordingly a wooden pier was speedily constructed, and immediately produced such manifest advantages to the inhabitants, by more than doubling the resort to the town, that converts to such an improved landing were daily made, and those steam packets which did not use the pier had but few passengers. The watermen and their families (calculated at 1500 persons) were in the greatest excitement; and though they menaced several of the parties engaged in the pier, they refrained from acts of open violence, in the full conviction that Parliament or the Admiralty, in the following session, would either suppress the pier, or that, during the winter, one of those misfortunes (predicted by them) of ships striking against it and lives being lost, would effectually and for ever remove it. The expectations of many persons were greatly raised by the fact of the *United Kingdom*, of 400 tons burthen, being totally wrecked in the month of January 1833, at Northfleet, about two miles to the west of the pier, and being for three days carried by the tide up and down the river between Northfleet and the east end of Gravesend; but, contrary to all expec-

tation, the pier sustained no injury; and thus was the fact fully established that, so far as the ebb and flood tides were concerned, the pier projection was no impediment to the navigation.

In 1833, a Bill was again brought into Parliament, and again opposed; but at length, upon the Committee in the Commons passing the Bill, the watermen, aided by several of the inhabitants, proceeded in the night to destroy the pier, which they effectually did, after extinguishing the lights in the town, by cutting away with saws and hatchets, the piles that supported the platform. Such was the violence and menace used, that the civil authorities were utterly unable to put a stop to the destruction of property, or to preserve the peace of the town; and the presence of the military from Tilbury Fort, the Rifle Brigade from Chatham, and the Cobham Yeomanry were found necessary for several weeks, whilst the pier was repaired and order restored.

By this act of violence the opponents of the pier lost much of their Parliamentary support; and the Duke of Buccleuch, the Lords Salisbury, Strangford, Faversham, and Wynford, with many others who had opposed the Bill, now gave it their support, and ultimately the Act passed, in June 1833; and on the 29th of July of the following year, within 13 months afterwards, the present pier was opened to the public.

The advantages to Gravesend consequent on this event have been immense, and very far exceed the calculations upon which the claim for a pier were founded. The resident population is now more than 17,000, and in the summer season it exceeds 20,000. The buildings have increased in the like proportion, and the coaches between Rochester, Maidstone, &c. and Gravesend, attending the arrival and departure of the steam packets, average forty daily throughout the year.

The residents and visitors each month are found to be gradually increasing, whilst the latter are no longer confined to the mechanic and artisan, but they consist of all classes of society, and thus become a source of wealth and prosperity to Gravesend and the adjacent district of Milton, and afford great gratification to all who had to endure the labour and trouble of obtaining for the town its present pier; whilst the public, instead of paying sixpence each for the compulsory use of a small, inconvenient, not to say dangerous landing wherry, pay but threepence toll for a safe and commodious pier, which payment of toll is included in the fare of the steam-packets.

The following statement of the numbers of passengers landing and em-

barking at Gravesend for the last ten years, will prove the rapid and perhaps unparalleled increase, caused by the combined advantages offered by steam navigation and a commodious pier:—

1829 . .	122,880	Before a pier was erected.
1830 . .	225,600	
1831 . .	237,600	
1832, 1833 . .	479,280	
1834 . .	313,896	
1835 . .	809,169	Since the pier has been erected.
1836 . .	834,803	
1837 . .	784,763	
1838 . .	872,721*	
1839 . .	1,005,430*	

GENERAL DESCRIPTION OF THE PIER.

The nature of the communication betwixt the quay and the water, previous to the erection of the pier which forms the subject of the present communication, will be fully understood from the plans and sections (Plate I.) Stairs descended from the quay wall, and from the bottom of the stairs a jetty extended to the low-water line of spring tides, or to a distance of about 116 feet from the front of the quay wall. The pier extends to a distance of 161 feet from the front of the quay wall, at which distance there is a depth of about 6 feet at low-water spring tides, and of 27 feet at high-water spring tides.

The centre line of the pier is represented by the line A B b, and the nature of the site and the extent of the erection will be seen at once from the general plan and longitudinal section of the foundations (Plate I.) The ground upon which the pier was to be founded was very limited, and the existing quay was to form part of the new pier. Also a separate jetty and stairs were to be provided for the use of the watermen. The general arrangements by which these objects were effected, are shewn by the elevation and general plan of the quay and pier (Plate II.) The pier consists of two parts, the promenade and the T head. The promenade, or part of the pier between the T head and quay is

* These numbers for 1838 and 1839 may not be strictly correct, but they are derived from the best sources of information that could be obtained.

127 feet in length, and 39 feet 6 inches in breadth, and supported by four arches resting on columns, as will be described hereafter. The T head, so called from its shape, on the extreme or northern end of the pier, is 29 feet 7 inches in width, and 73 feet in frontage to the river. It is supported on eighteen cast-iron columns. The descent to the stairs for embarking and disembarking from the vessels, is in the centre of the extreme end of the pier (Plate III.)

Watermen's Causeway. The causeway at which the watermen land at all states of the tide is 111 feet 6 inches in length, and 12 feet 5 inches in breadth, supported on seven cast-iron frames resting on separate foundations, and on transverse bearing beams attached to the cast-iron columns which support the promenade, four of which are provided with bracketed flanches for this purpose. The ground being excavated to a sufficient depth to secure a good foundation, an entire course of 3-inch York paving-stone, well bedded in mortar, and set quite level, was laid, holes having been previously cut through the stones for receiving the hold-down bolts; upon this the brickwork was laid, capped with Bramley Fall stone, 18 inches wide and 12 inches thick, with holes for the hold-down bolts to secure the frames which support the causeway. The end of the causeway near the quay wall, and the steps up to the quay, are supported on three similar foundations of brickwork parallel to each other, the intermediate spaces being filled up with concrete (Plate I.)

The cast-iron frames are firmly secured down to the brick and stone-work by four iron bolts, passing through the foundations, and the transverse beams are fitted endways between the columns, and screwed down to the flanches by 1-inch screw-bolts and nuts; longitudinal fenders of oak timber, 9 inches square, are fixed at the outside corners of the frames, and to the transverse pieces between the columns by 1-inch screw-bolts and nuts; between these longitudinal fenders are three cast-iron beams, each of which rests with one end on one half of the transverse beams between the columns, and is supported at the other end and at intermediate points on the frames, and securely fixed by dovetail keys. The whole is covered with 4-inch York paving.

Foundations of columns of Promenade. The promenade rests on cast-iron ribs forming four arches, supported by iron columns on brick and stone foundations (Plates I. & II.) The ground being excavated to a good and sound bottom, the entire surface of the excavation is laid with Bramley Fall rag-stone, dressed 9 inches in thickness, and no stone less than 4 feet square, set level in every direction, and well bedded in mortar. The stones are pierced with holes 4 inches square, for

receiving the hold-down bolts, each of which is provided with a cast-iron plate 15 inches square, well bedded to the under side of the stone. The brickwork being carried up to the proper height, is capped with two Bramley Fall stones, each 5 feet 9 inches square and 2 feet thick. Holes are also bored through these, the vertical spaces left in the brickwork being made to correspond with the holes in the lower stones, and great care was taken in setting out the holes and keeping the bolts in a perpendicular direction, and so wide as to correspond accurately with the centre of the bolt-holes in the bottom of the columns.

The sum to be expended by the Corporation on the new pier, was not sufficient to admit of the use of caissons or a coffer-dam, and the obstruction which they would have caused to the navigation was an additional objection to their use. Recourse was consequently had to a more simple mode, which has proved from experience equally advantageous and far less expensive, and which, it is believed, was never before practised. The complete success of the method, and the great advantages which may attend its adoption in particular cases, will be seen from the following account.

Foundations of T head.

The columns of the T head are supported on fifty-four cast-iron piles, each column being supported on three piles. The stratum into which the piles are driven, consists chiefly of chalk, and as it was of the greatest importance that this part of the work should be executed with accuracy, in order that the centres of the piles, when driven, should correspond with the centre of the holes in the flanches at the bottom of the columns, the following precautions were adopted. A large platform of whole timbers was formed in length, breadth, and dimensions, as shewn in the plan (Plate I.), and well secured by 1-inch screw-bolts and nuts. Upon this, was accurately set out the distances between the centre of each column forming the T head of the pier. Plates of cast-iron $1\frac{1}{2}$ inch thick, eighteen in number, and each having three circular holes, $17\frac{1}{2}$ inches diameter, called guide-plates, were then screwed down in their proper situations upon the platform; the centres of the holes in each plate being adjusted so as to correspond exactly with the centre of the site for each pile.

The platform was then floated out and secured by moorings in its proper place, at the level of low-water spring tides, until four piles were driven, one at each corner, for securing it more permanently.

This being done, a cast-iron shell was introduced perpendicularly into one of the three holes of the guide-plates, and driven through the mud into the chalk below. An auger was then inserted, varying from nine to ten inches in

diameter, according to the density of the chalk, and the boring was proceeded with to within one foot of the length of the pile intended to be used. The depth of the boring varied according to the length of the piles, and other circumstances; the pile was then introduced through the shell into the bored hole, and driven down to within two or three feet of the top of the shell, when the shell was drawn up, and the pile driven down by means of a hard wooden dolly, made to fit the top of the piles, and upon which the monkey of the pile-engine acted. The greater portion of the piles for the T head were driven in this manner under water, in consequence of the prevailing wind during the months of February, March, and April, being easterly, which prevented the tide ebbing out to its usual extent, so that the platform and piles were seldom seen except when the wind shifted to the west, and oftentimes when it did it was for so short a period, that little could be done before the return of the flood. Under these circumstances, as it was evident that if favourable winds were waited for much time would be lost, it was determined to remove the platform. This having been removed, a wooden cylinder, 9 feet diameter, and 9 feet long, was made of 3-inch deal battens, firmly keyed and hooped together, the lower end being shaped like a sheet pile, and shod with iron. This cylinder was lowered over one set of three piles, and loaded sufficiently to cause it to sink through the soft mud of the shore, when it was driven into the hard ground. The water was then pumped out, and the mud removed low enough to enable the workmen to reduce the heads of the piles to a uniform level by chipping, so that the bases of the columns were fitted down to the tops of the piles metal and metal, and this operation was repeated as often as required. But the piles were found to have been driven with such accuracy, that not more than six required chipping, and the greatest variation did not exceed $\frac{3}{4}$ of an inch.

Adjusting Plate and Columns. When the pile heads of each set were adjusted, a cast-iron plate, connecting the three piles together, called the adjusting plate, was firmly keyed on immediately below the top of the piles (Plate IV.) The columns were then fitted down to the adjusting plate by $2\frac{1}{2}$-inch screw-bolts through flanches at the bottom. The holes in these flanches were a little elongated, so as to allow of the columns being slightly shifted and fixed in their true position (Plate IV.) Upon the columns rest the deep bearing-beams which support the superstructure. These bearing-beams are screwed together at the corners by $1\frac{1}{2}$-inch screw bolts, and fastened by bracket flanches to the capitals of the

columns; on their upper sides are flanches on which the flooring of the T head is laid. The columns of the T head are tied together at about one-third of their height, by timbers fastened to bracketted flanches on the columns. On these timbers are supported the frames for the stairs in the T head. The details of the adjusting Plate, the columns, the deep bearing-pieces, and the stairs, are fully shewn in the accompanying Plates.

Arches of Promenade. Upon each of the columns of the promenade rests a cast-iron rib 40 feet in length; each arch is composed of two of these ribs, secured together at the centre by $1\frac{1}{2}$-inch screw bolts and nuts. The whole structure consists of four such arches, kept apart at their proper distances by means of distance pieces (Plate VI.), nicely fitted to the ribs, and secured by $1\frac{1}{2}$-inch screw bolts. The ends of the ribs next the quay are secured by means of a square flanched piece built into the wall, and by hold-down bolts; four horizontal and vertical holes being made through the springing of the land arch, for this purpose. The ribs at the other end of the promenade, form part of the T head, as well as one-half of the arch, and are secured to the columns and deep bearing-beams. The different portions of the iron-work of the arches, and superstructure of the T head, were fitted together in a temporary manner at the iron-foundry, and when it came to be fixed upon the columns, scarcely any chipping was found necessary; indeed, many of the holes in the capitals of the columns were drilled from the dimensions of the working drawings, and found to correspond with the holes in the flanches of the deep open beams.

Floorings, Stairs, and Superstructure. The top edge of the arches of the deep bearing-beams of the T head being perfectly level, the wooden flooring-beams were laid on the flanches, and screwed down by 1-inch screw bolts. The stairs of the T head are supported in iron frames resting on the two pieces fixed to the flanches on the columns. Oak steps are laid on the flanches of these frames, and screwed down with $\frac{5}{8}$-inch counter-sunk bolts and screws. The bottom and top of these iron frames are secured to their respective bearing-timbers, and to the side of the columns.

The promenade and T head are surrounded by a cast-iron fence, the small pillars of which are half an inch in thickness, and the longer pedestals $\frac{5}{8}$ths of an inch, all having flanches at the bottom for fixing them perpendicularly and true, one with the other, in every direction, by means of $\frac{3}{4}$-inch screw bolts and nuts. Through the small pillars are cast 3 holes 3 inches deep, and $\frac{5}{8}$ths of an inch wide, to admit the horizontal fence-bars of wrought-iron to

pass at the back of the diagonal braces, which, as well as the handrail, are of Memel timber. The awnings at the east and west ends of the T heads, are supported by six ornamental columns, each an entire casting $\frac{7}{8}$ths of an inch thick, and screwed down by eight $1\frac{1}{8}$-inch screw bolts and nuts.

The bearing-timbers forming part of the cornice round the exterior and interior of the T head, and on each side of the promenade, over the arches to the quay wall, are 6 inches deep and 12 inches wide. These timbers are secured to the cast-iron work with $\frac{7}{8}$-inch screw bolts and nuts passing through the cast-iron flanches for fixing the cornice. From the centre of the T head to the termination of the promenade, run longitudinally five bearing-timbers, secured nine inches into the quay wall, and to their respective flanches by $\frac{7}{8}$ screw bolts and nuts. The whole area of the promenade, and of the T head, is then covered with 4-inch Memel plank, in proper lengths for breaking joint; a plank at every ten feet apart extending the entire width of the promenade.

The framing of the roofs of the awnings and the turrets are shewn in the drawing (Plate VI.); the acroteria forming the ornaments or tiles at the top of the cornice are of cast-iron, and firmly screwed to the cornice. The roofs of the awnings and turrets are covered with copper 16 oz. to the foot.

Lighthouse. Long after the plans had been matured, and the castings made, it was determined to have a cast-iron column to exhibit a night-light (Plate III.) This column is situated at the centre of the pier on the line of juncture of the promenade with the T head. It is 30 feet in height, 3 feet in diameter at the bottom, and 2 feet 6 inches diameter at the top. It has a staircase in the centre, and was cast in three pieces. The lantern is glazed with red glass, and lighted by gas.

DESCRIPTION OF THE PLATES.

Plate I.

A general plan of the approaches and site previous to the erection of the pier, shewing the town-wall and quay; the stairs and bridge, or jetty, leading down to the river; quoting the distances from the town and quay wall, and the depths of water at different points, at low-water of spring-tides.

Sections on different lines of the plan, shewing the levels and depths of soil and water with reference to low-water spring-tides.

Scales, shewing the rise of the tide at Gravesend pier, and at the entrance lock of the Thames and Medway Canal.

A general plan of the foundations of the new Pier; shewing the foundations next to the quay-wall, and the bolt-holes for fastening the frame-work to carry the steps leading from the watermen's causeway to the quay. Also the seven other foundations for the iron framings which support the causeway. On the first foundation wall is shewn in section the iron framing; the others only shew the bolt-holes. The foundations of the columns of the promenade. The frame-work for fixing the piles, the guide-plates being shewn on one half, and the piles on the other half. The longitudinal section of the foundations, and a section of the columns and piles, and the temporary frame-work.

Plate II.

Side elevation and general plan of the pier.

Side and end elevation, and plan and dimensions of the half ribs which form the tie with the quay-wall. This is bolted down to the foundation-plate in the quay-wall by vertical and horizontal bolts (*see* p. 253), the former of which is shewn at the end of the flanch in the elevation.

Elevation of the half rib, forming part of T head. At A, B, C, are the distance pieces, the details of which are given in Plate V. At A is shewn the union of two ribs at the centre, the bolts passing through the transverse pieces. At D is the end of the deep-bearing beam of the T head.

Elevation of a half rib and column of the promenade, shewing the bolts of the distance pieces.

Plate III.

The river front.

Plate IV.

Details of the columns, piles, guide plates and adjusting plates, with the dimensions.

Details of the deep-bearing beams, forming the T head. The situation of these in the plan of the structure is shewn by A, B, C, in Plate VI.

Details of the steps at T head. The columns on the plan of the steps, marked *e*, *d*, *g*, *k*, are those referred to by the same letters in Plates V. and VI.

Plate V.

Details of the distance pieces, the situations of which are shewn by the letters A, B, C, Plate II.

Details of caps of columns and deep-bearing beams of T head. The columns *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *k*, are those shewn by the same letters in the plan on Plate VI. The column *k* being the lamp column, Plate III. The column *i* is not shewn on the plan Plate VI. but is one of the columns of the promenade, these being all alike. The columns in the other half of the T head occupy the same relative positions as those here described.

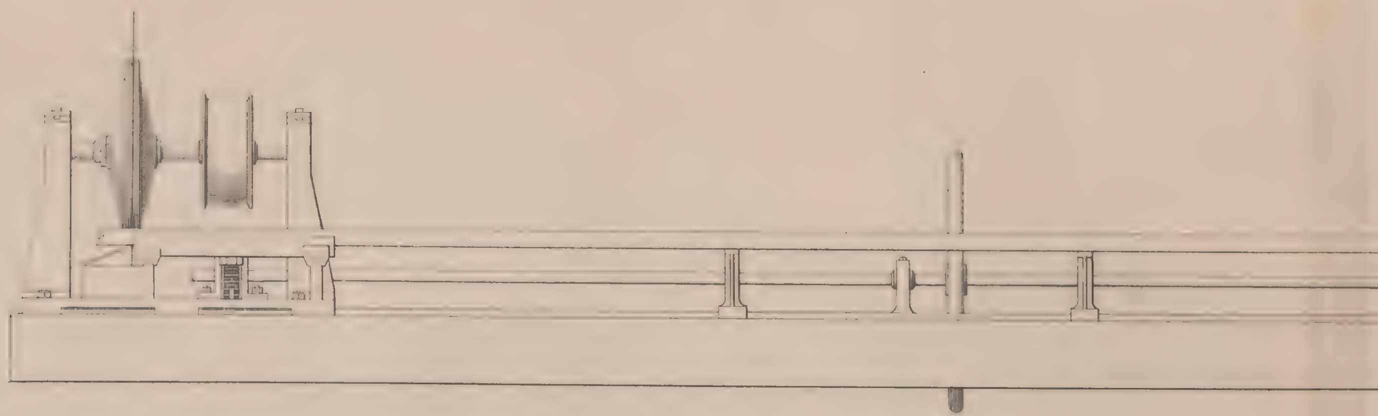
Plate VI.

Elevation of the awnings, or pavilions and turrets, and details of the woodwork of the superstructure.

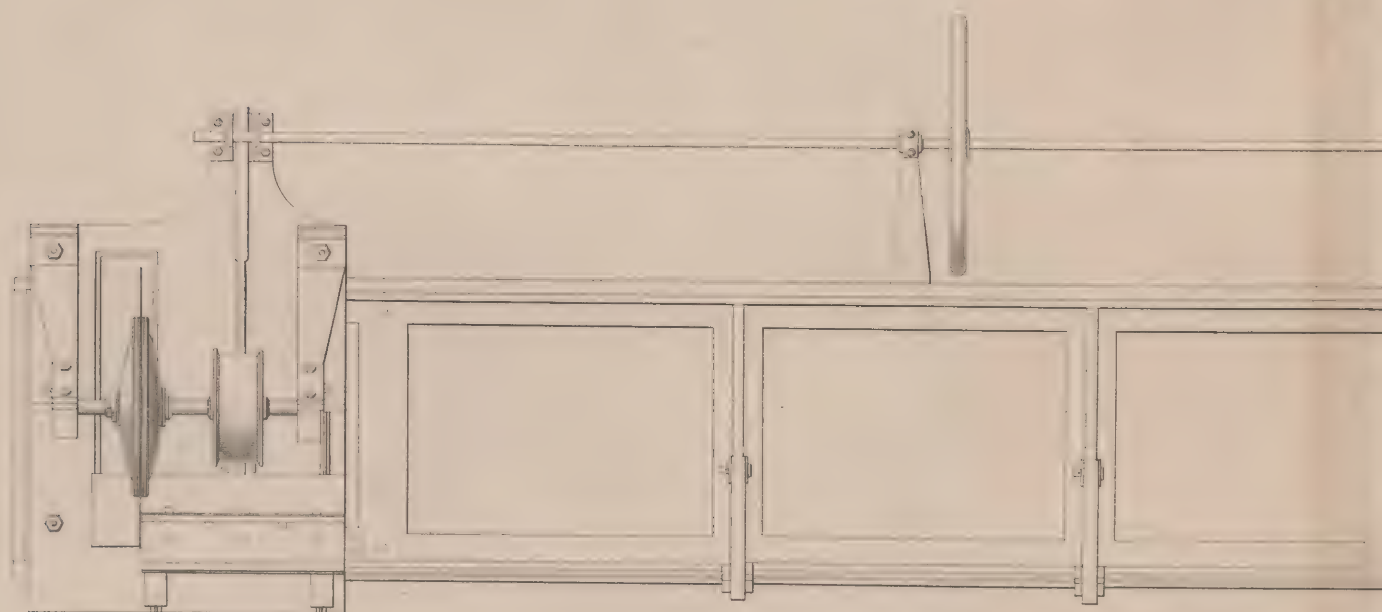
Plan of T head; one half shewing the manner of fastening the iron bearing beams to the caps of the columns, and the other half the plan of the timbers of the floor resting upon the iron beams. A, B, C, are the positions of the bearing-pieces, the details of which are shewn on Plate IV.; and *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *k*, the positions of the columns, the details of which are given in Plate V.

SAWING MACHINE FOR CUT

JOSEPH GLYNN

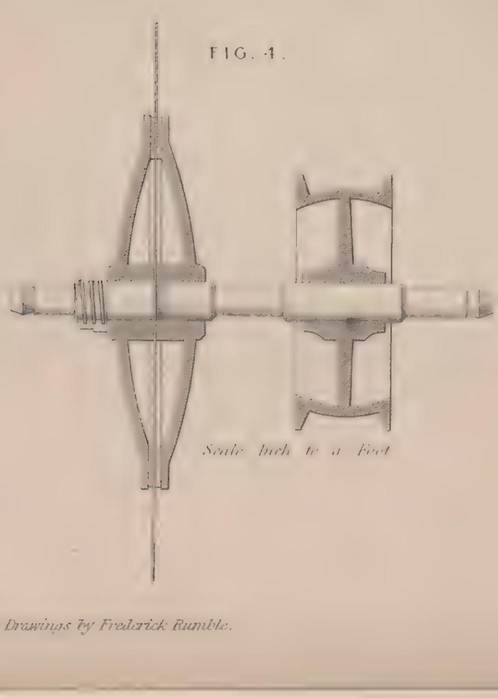


ELEVATION. FIG. 1.



PLAN. FIG. 2.

FIG. 4.



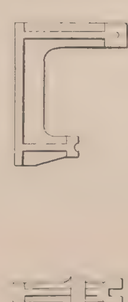
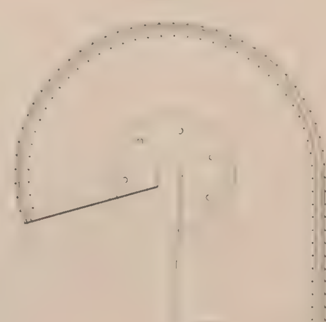


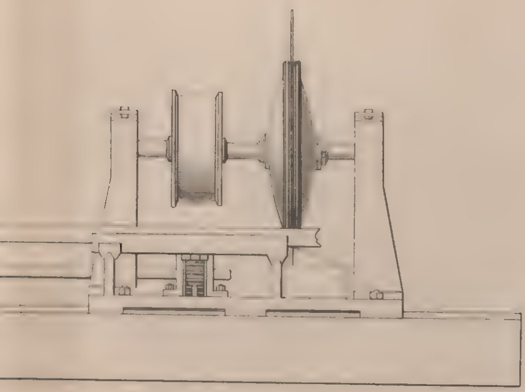
FIG. 6.

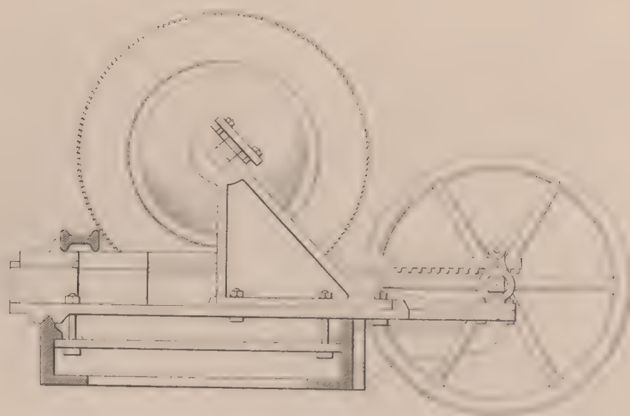


Scale of Feet for Figs. 1. 2. 3. 6. 7

CUTTING RAILWAY BARS. VOL. II.

PH GLYNN.





END VIEW. FIG. 3.

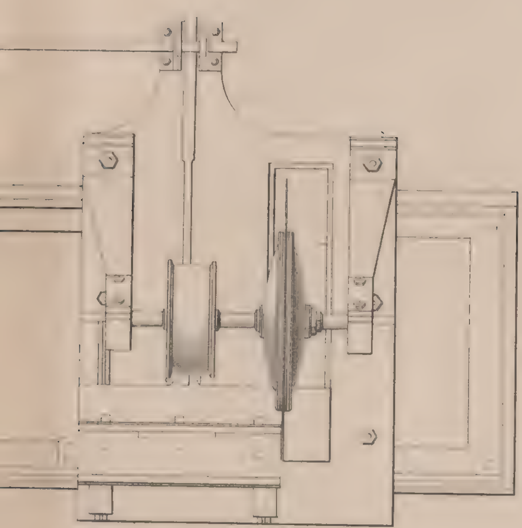
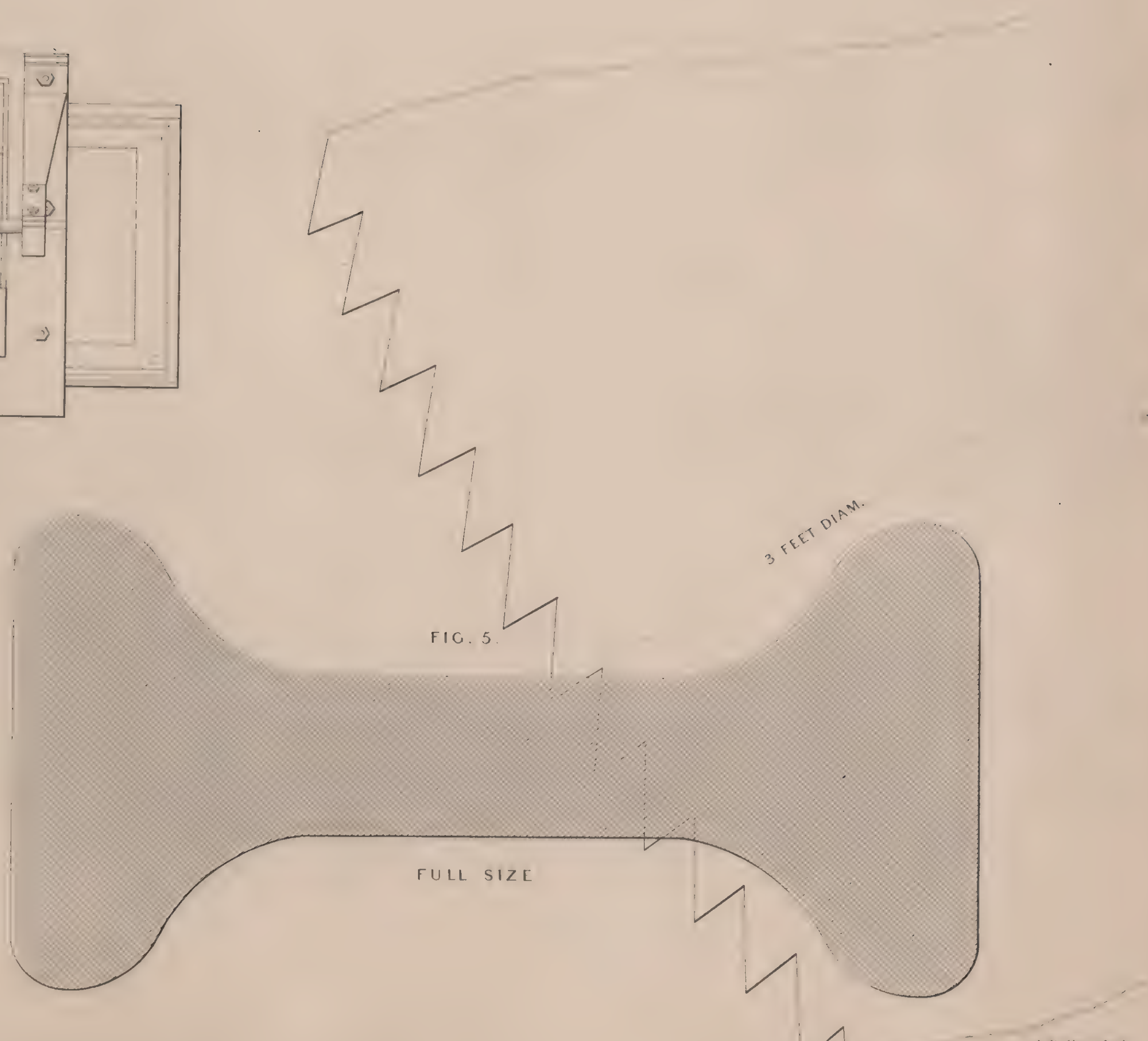


FIG. 7.



3 FEET DIAM.

FIG. 5.

FULL SIZE

Civil Engineers 1840. S. Bellin, sculp.

SKETCH OF THE SUSPENSION BRIDGE OF MONTROSE IN ELEVATION.

AS IT APPEARED AFTER THE STORM OF THE 11TH OCTOBER 1833.

C.W. PASLEY, C.B. COLONEL, R.E.

THE ORIGINAL LENGTH OF ROADWAY BETWEEN THE PIERS 412 FEET

FIG. 1.

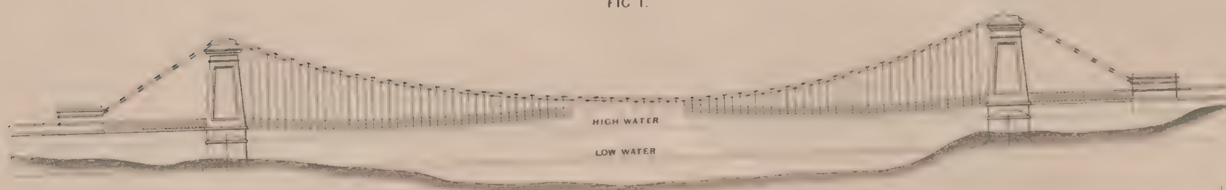
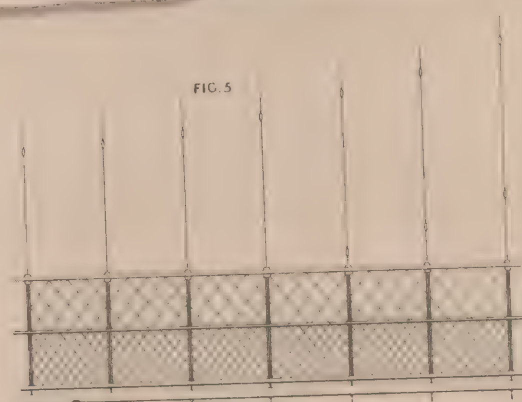


FIG. 2

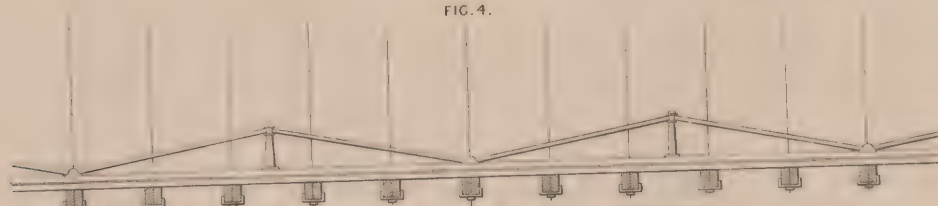
SKETCH OF THE SIDE RAILING OF THE BRIDGE AT MONTROSE

FIG. 5



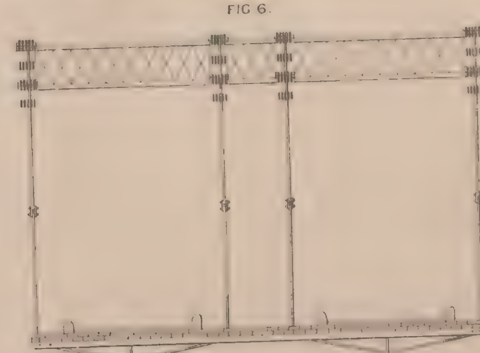
SIDE RAILING OF THE MENAI BRIDGE

FIG. 4.



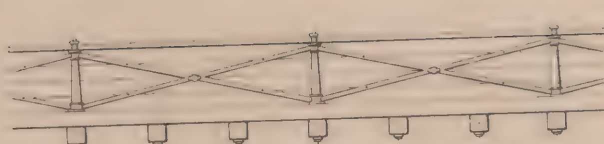
CENTRAL TRUSSING OF THE BRIDGE AT HAMMERSMITH

FIG. 6.



TRANSVERSE SECTION OF THE MENAI BRIDGE

FIG. 3.



SIDE RAILING OF THE BRIDGE AT HAMMERSMITH

SCALE FOR FIGS. 2 3 4 5 AND 6

10 5 0 10 20 FEET

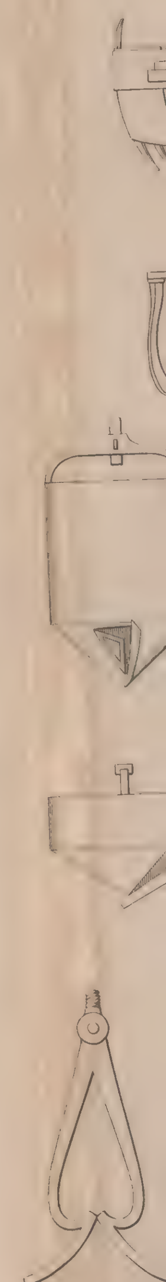
Reduced from the original Drawings by Mess.rs Jones & Lomax.

SECT

NE

AT THE HA

DRAWINGS



Institution of Civil Engineers 18

SECTION OF AN ARTESIAN WELL

SUNK BY THE

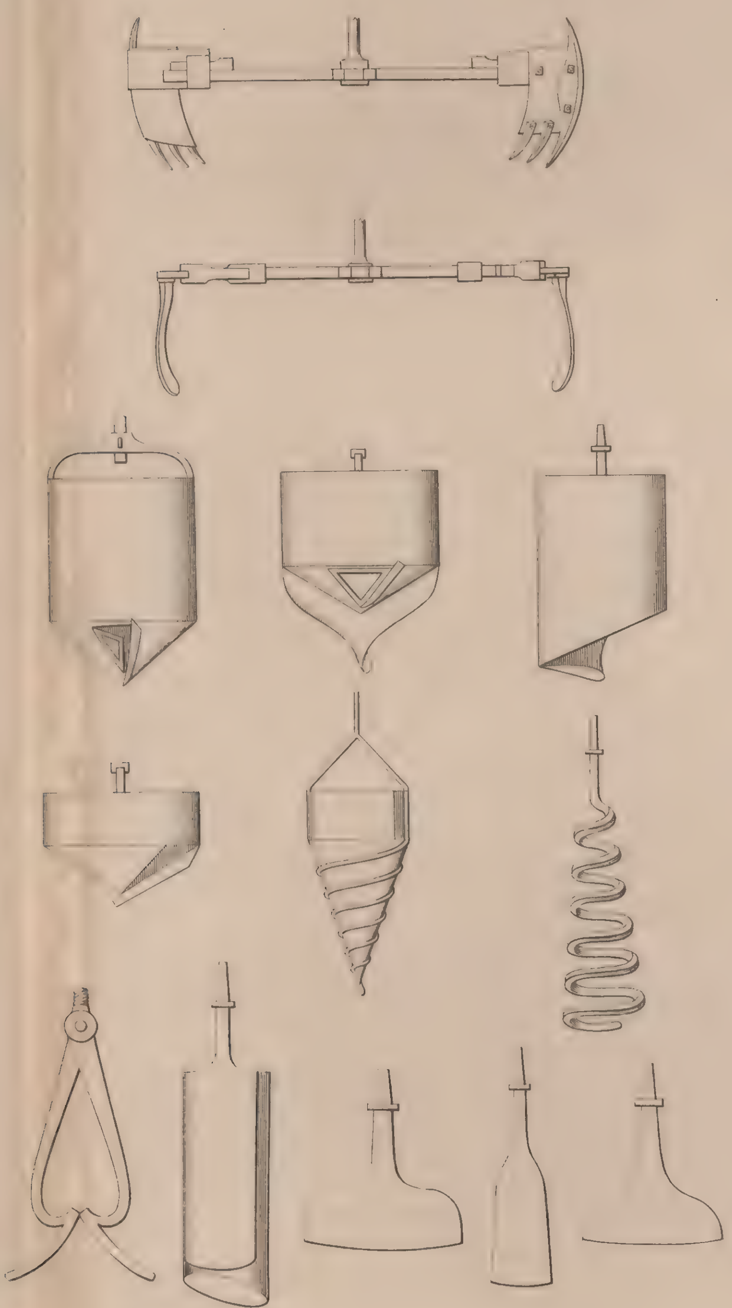
NEW RIVER COMPANY

AT THE HAMPSTEAD ROAD RESERVOIR, LONDON.

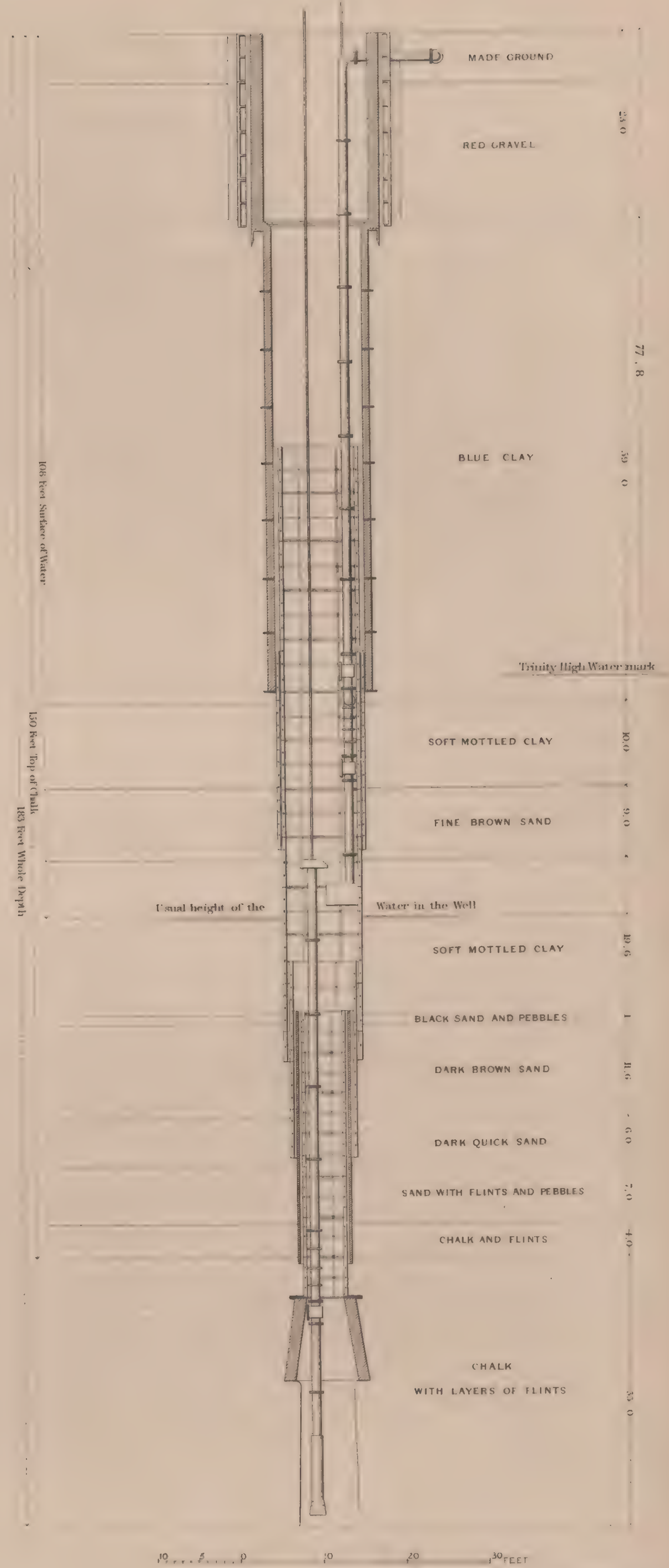
WITH

DRAWINGS OF THE TOOLS USED IN ITS EXECUTION.

W^m. C. MYLNE.



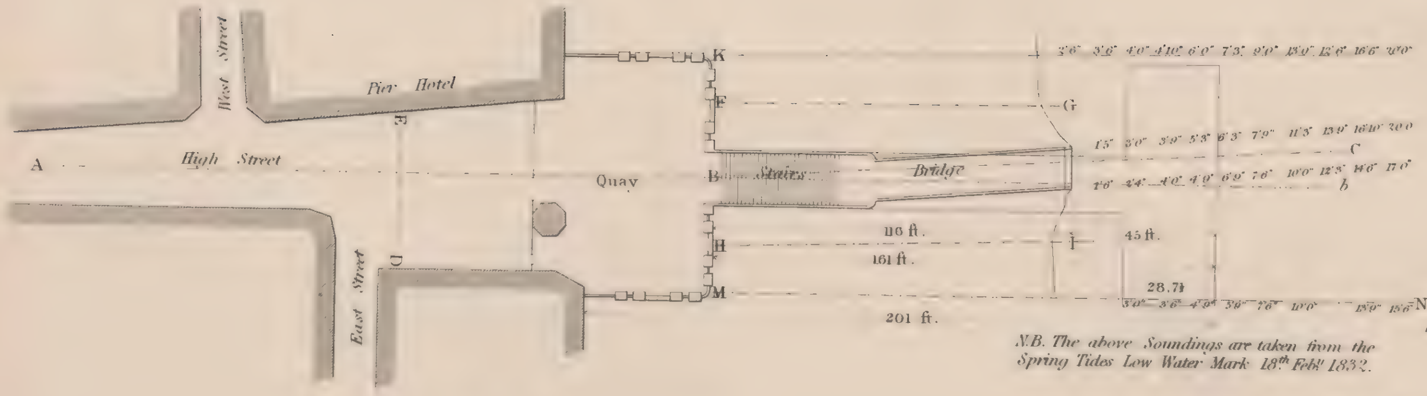
SECTION



J.W.Lowry sculp.

GRAVESEND PIER,

W. TIERNEY CLARK.



N.B. The above Soundings are taken from the Spring Tides Low Water Mark 18th Feby 1832.

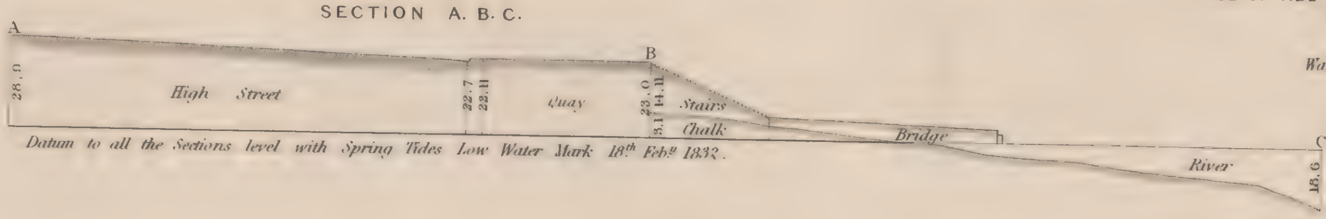
RISE OF TIDE AS TAKEN AT GRAVESEND PIER,
18th February 1832.

High Water Mark 20.0 17th February.
20.3 18 Do.

18,9	2 O'Clock	17 Minutes
17,9	2	1
16,8	1	48
15,8	1	33
14,8	1	18
13,7	1	2
12,0	12	37
10,5	12	12
8,10	11	50
7,4	11	30
5,6	11	10
4,2	10	53
2,6	10	30

Low Water Mark 18 Feby 1832.

SECTION A. B. C.



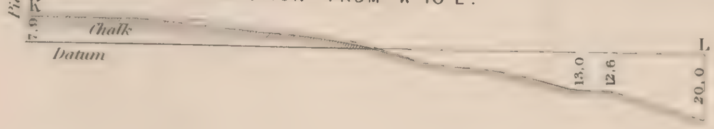
RISE OF TIDE AS TAKEN OUTSIDE THE ENTRANCE LOCK TO THE THAMES AND MEDWAY CANAL,
17th February 1832.

Wall Top of Quoin 23,0 West Side of Lock Gate
Level with 23,0 Gravesend Pier.

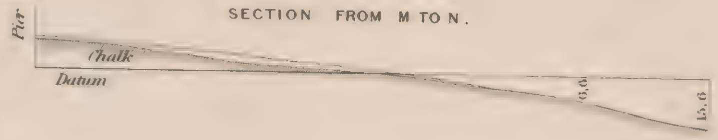
20,9	2 O'Clock	0 Minutes
20,0	1	32
19,0	1	15
18,0	1	1
17,0	12	49
16,0	12	34
15,0	12	19
14,0	12	4
13,0	11	53
12,0	11	37
11,0	11	17
10,0	11	3
9,0	10	50
8,0	10	40
7,0	10	30
6,0	10	15

New Sill, entrance 1,6 to Canal Lock.
Old Sill, at Do. 1 Inch above Low Water Mark
17th Feby 1832.

SECTION FROM K TO L.



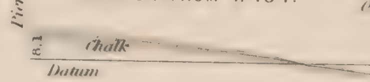
SECTION FROM M TO N.

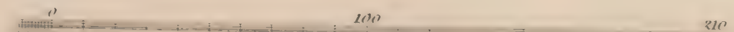


SECTION FROM F TO G.

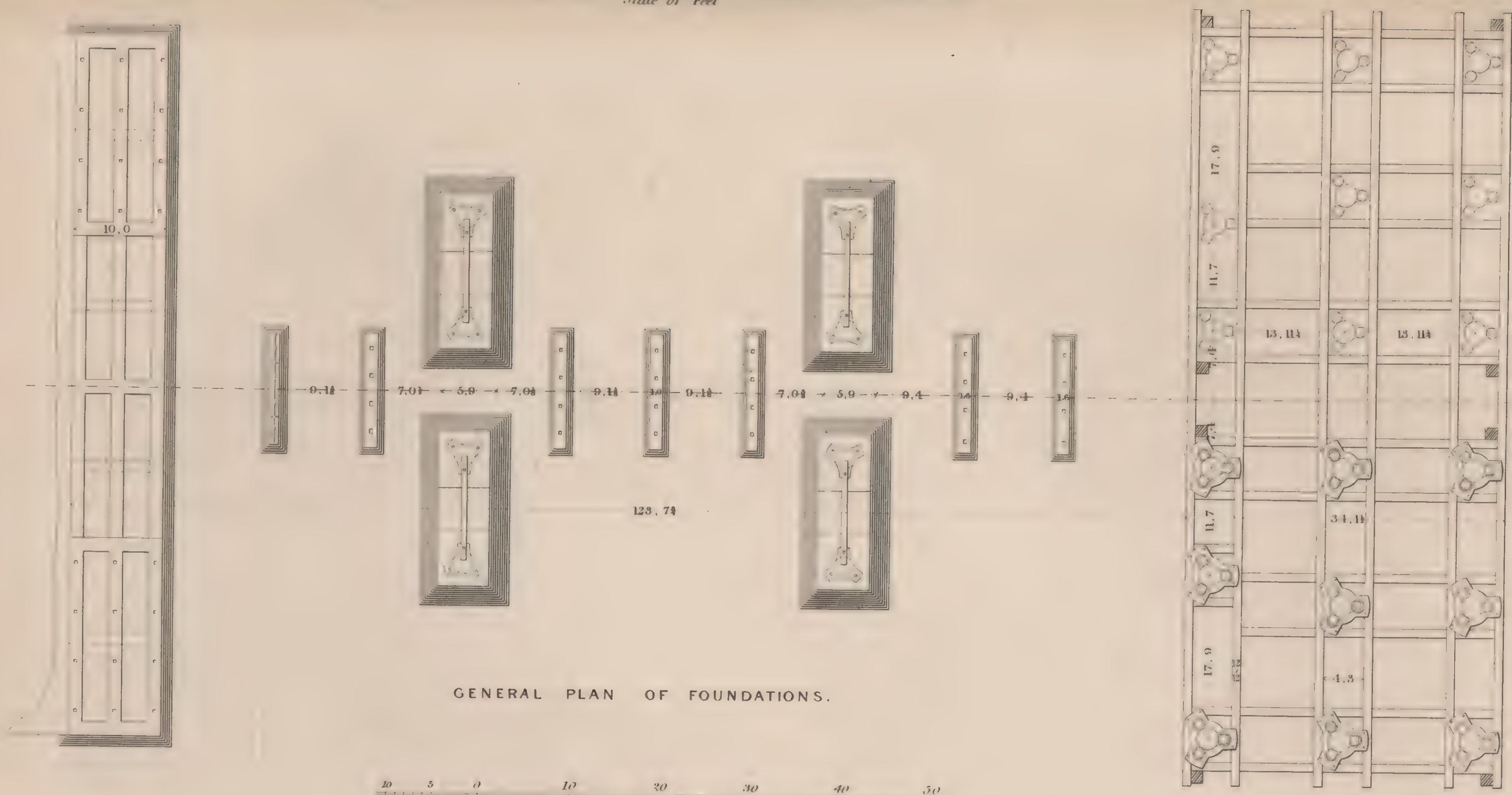
SECTION FROM E TO D.

SECTION FROM H TO I.

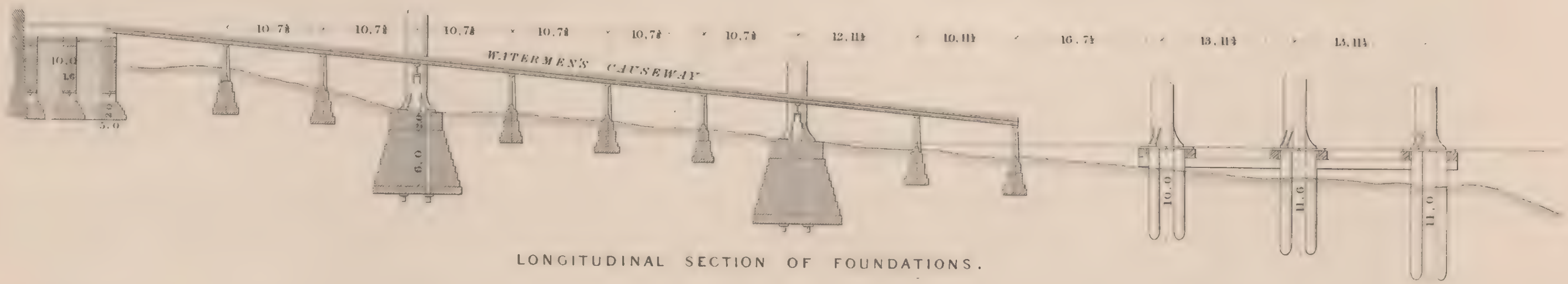




Scale of Feet



GENERAL PLAN OF FOUNDATIONS.



LONGITUDINAL SECTION OF FOUNDATIONS.

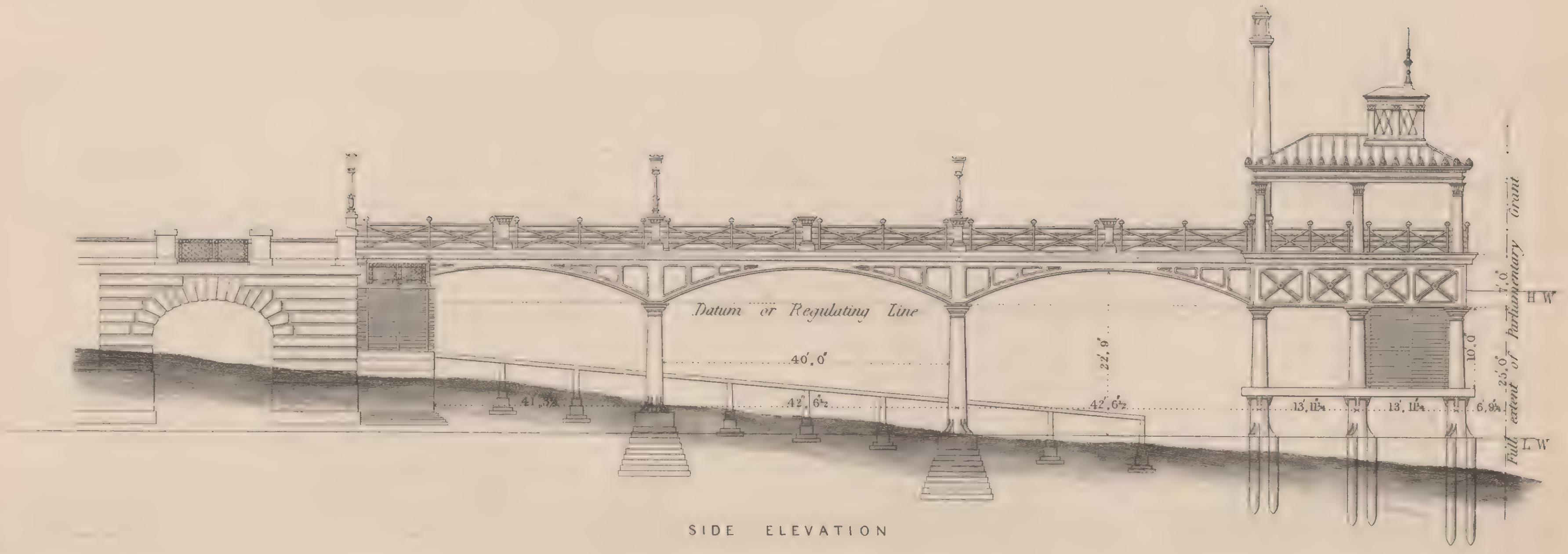
Reduced from the original drawings by Fredk. Rumble.

J. W. Lowry, sculp.

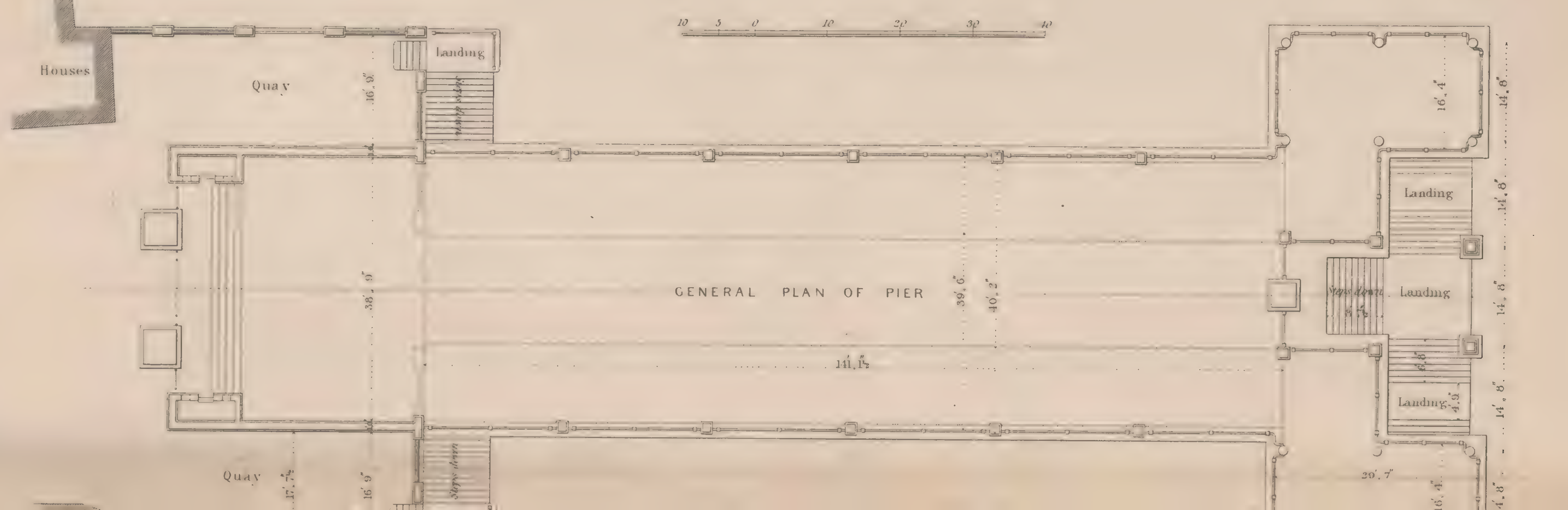
Institution of Civil Engineers, 1840.

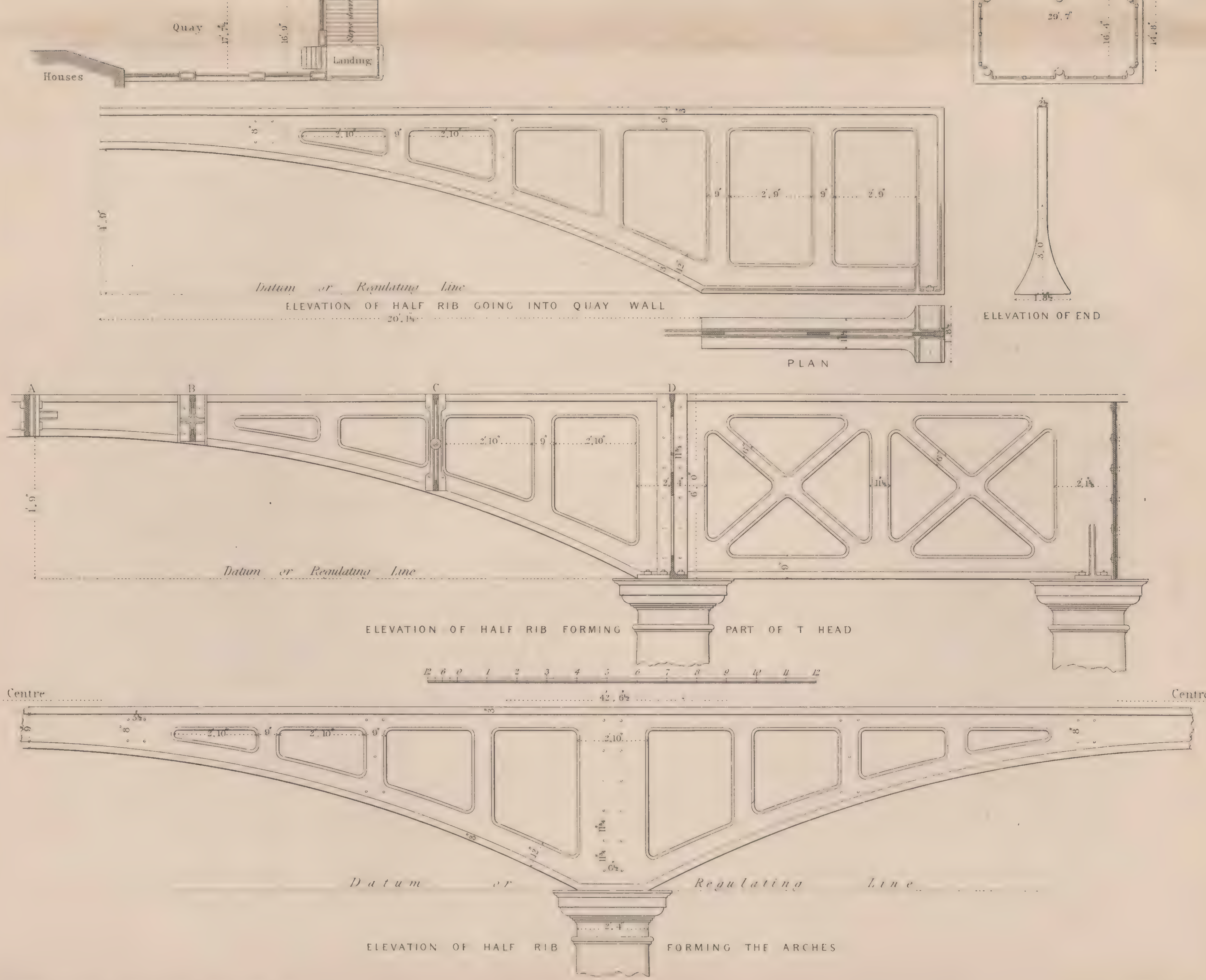
GRAVESEND PIER.

W. TIERNEY CLARK



SIDE ELEVATION





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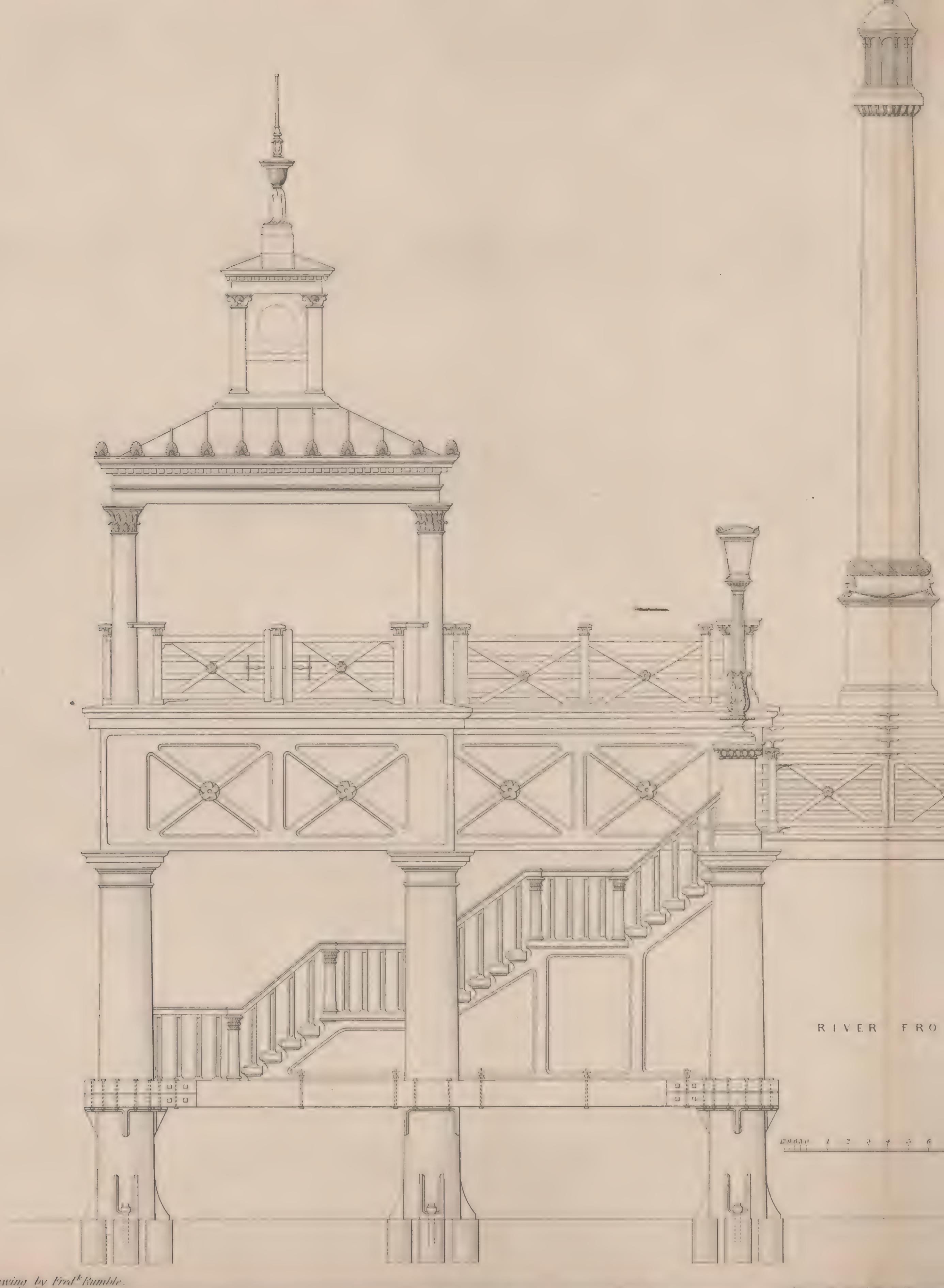
Institution of Civil Engineers, 1840.

J. W. Lowry, sculp.

GRAVESEND

W. TIERNEY CLARK

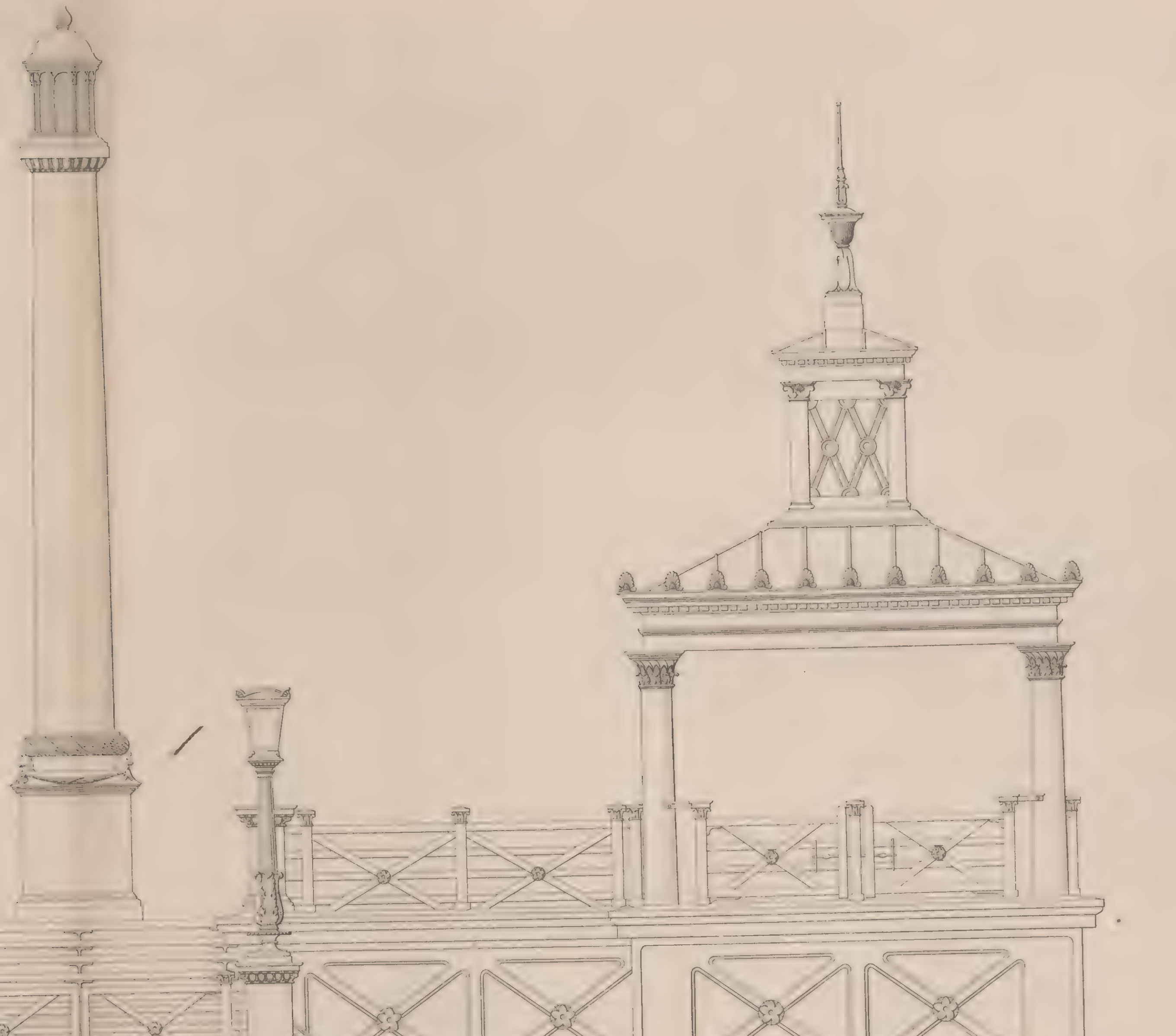


Reduced from the original drawing by Fredk Rumble.

ESEND PIER,

TIERNEY CLARK.

PLATE III

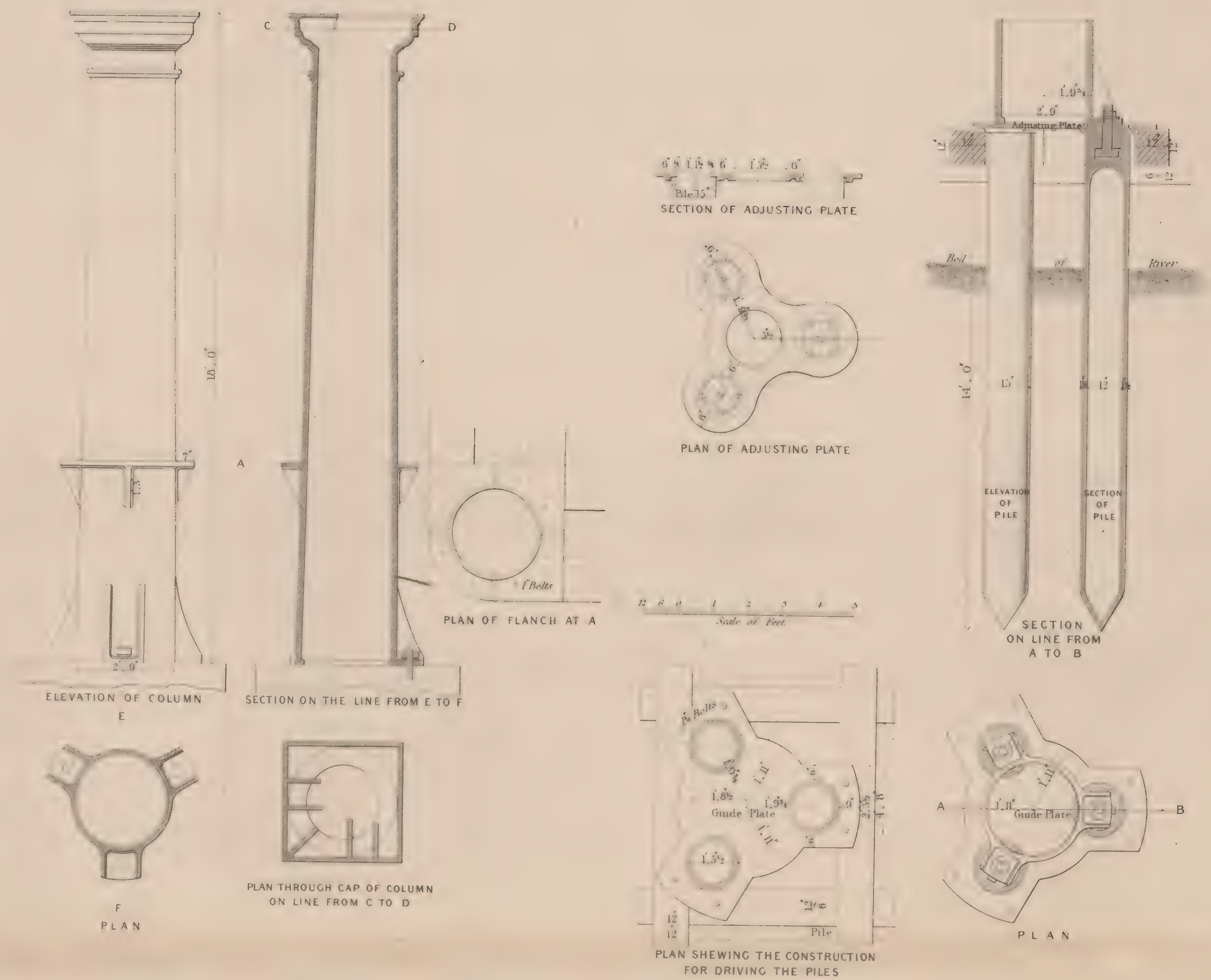


W. A. Beever, sculp

tution of Civil Engineers 1840.

GRAVESEND PIER,

W. TIERNEY CLARK

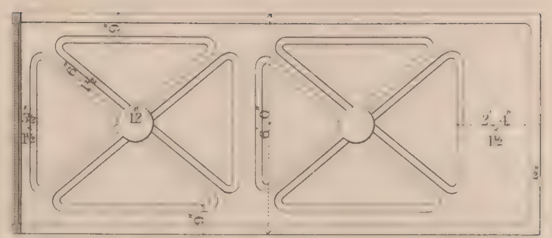


F
PLAN

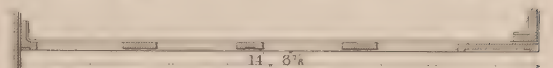
ON LINE FROM C TO D

PLAN SHEWING THE CONSTRUCTION
FOR DRIVING THE PILES

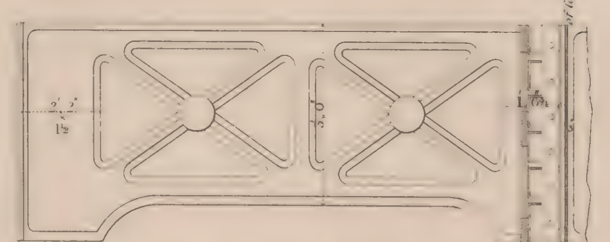
PLAN



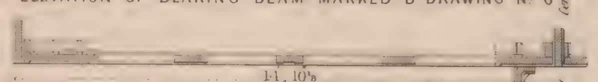
ELEVATION OF BEARING BEAM MARKED A DRAWING Nº 6



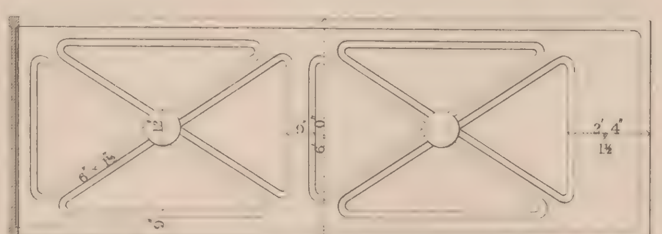
PLAN



ELEVATION OF BEARING BEAM MARKED B DRAWING Nº 6

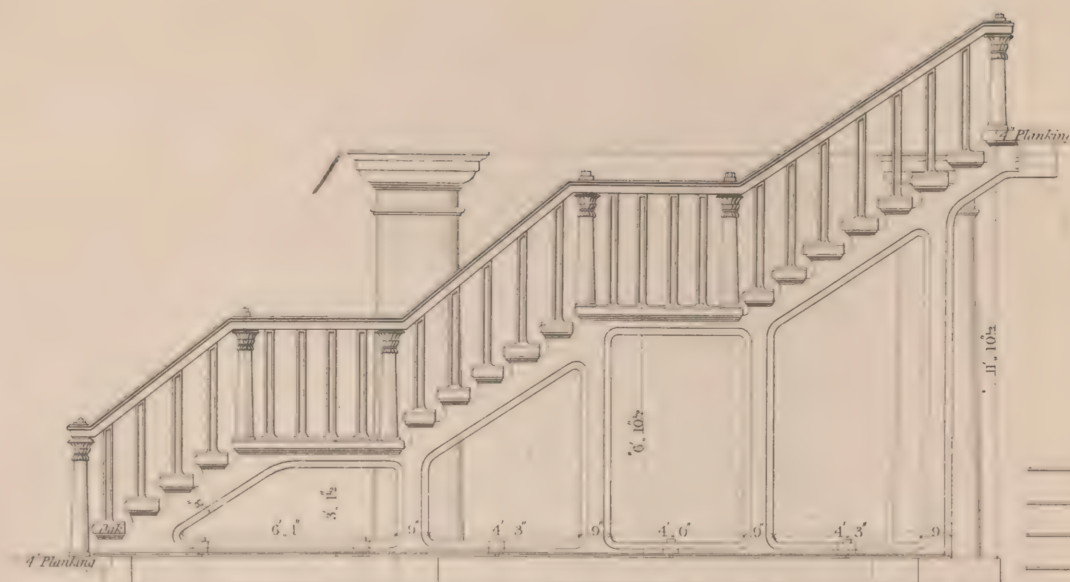


PLAN



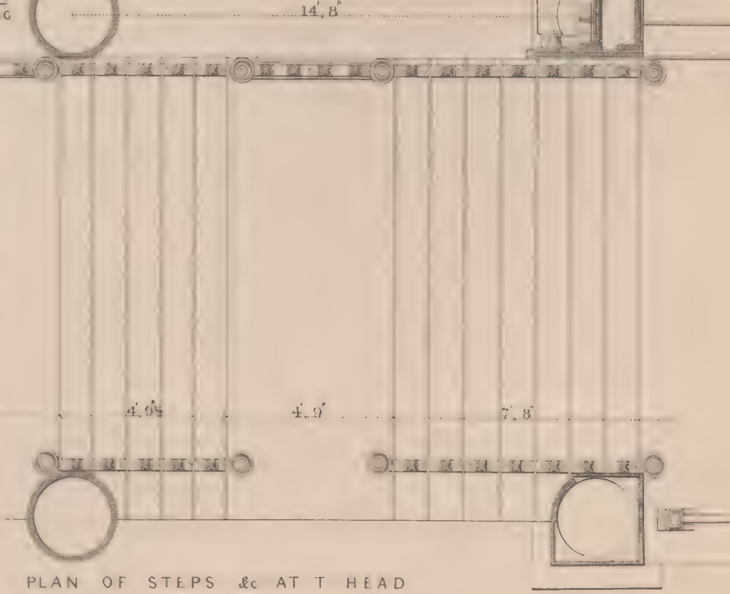
ELEVATION OF BEARING BEAM MARKED C DRAWING Nº 6

PLAN



ELEVATION OF STEPS &c. AT T HEAD

PLAN OF TOP PART OF HANDRAIL SHEWING THE MANNER OF FASTENING &c.



PLAN OF STEPS &c AT T HEAD

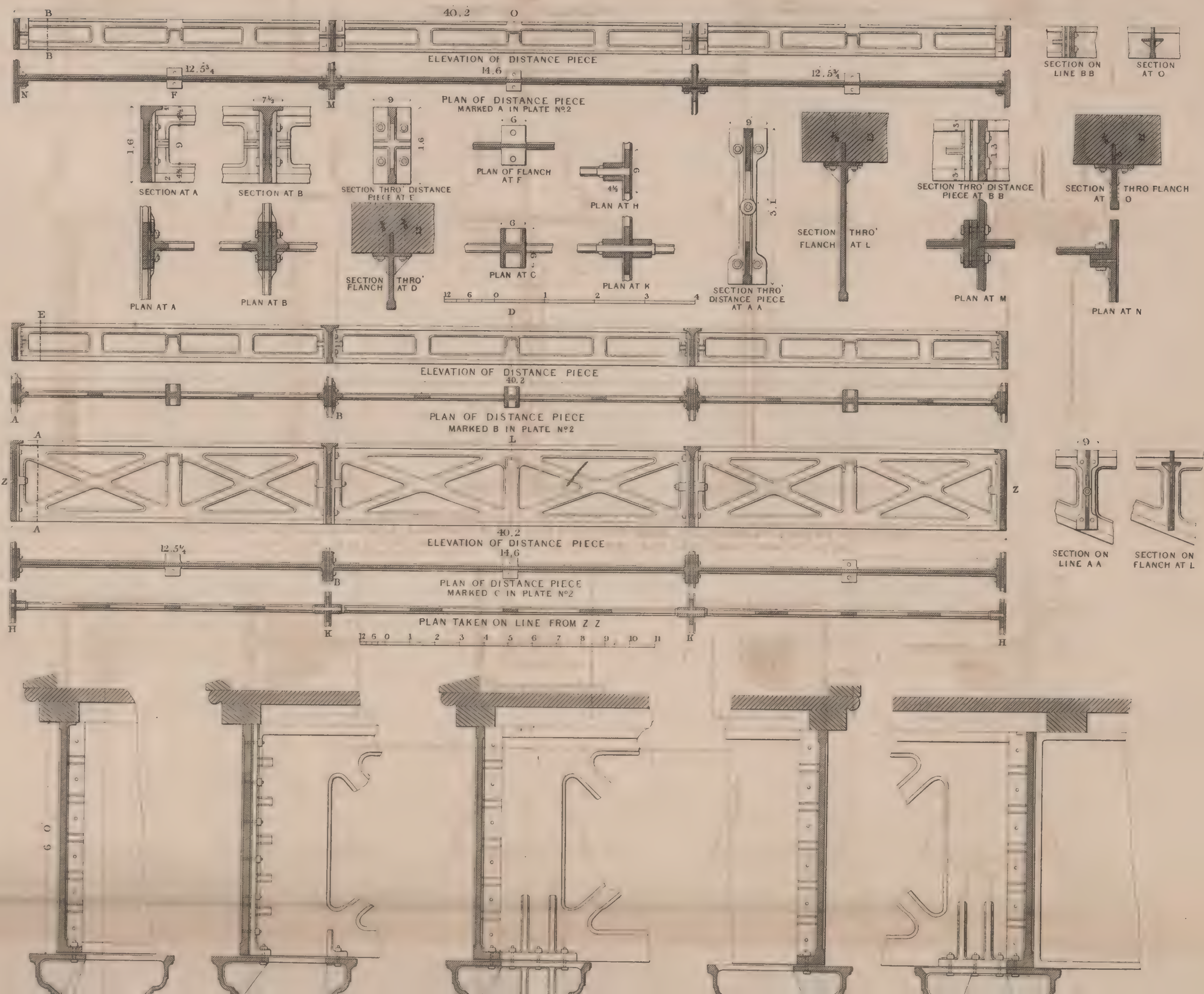
Reduced from the original drawings by Fredk Rumble

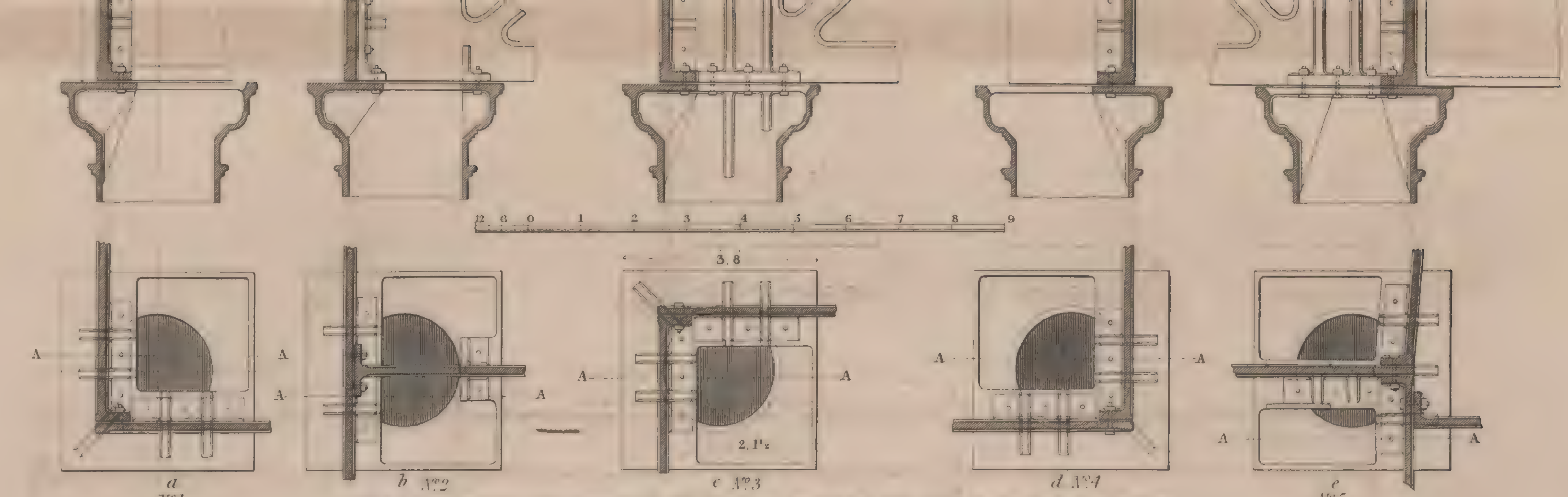
S. Bellin, sculp.

Institution of Civil Engineers 1840.

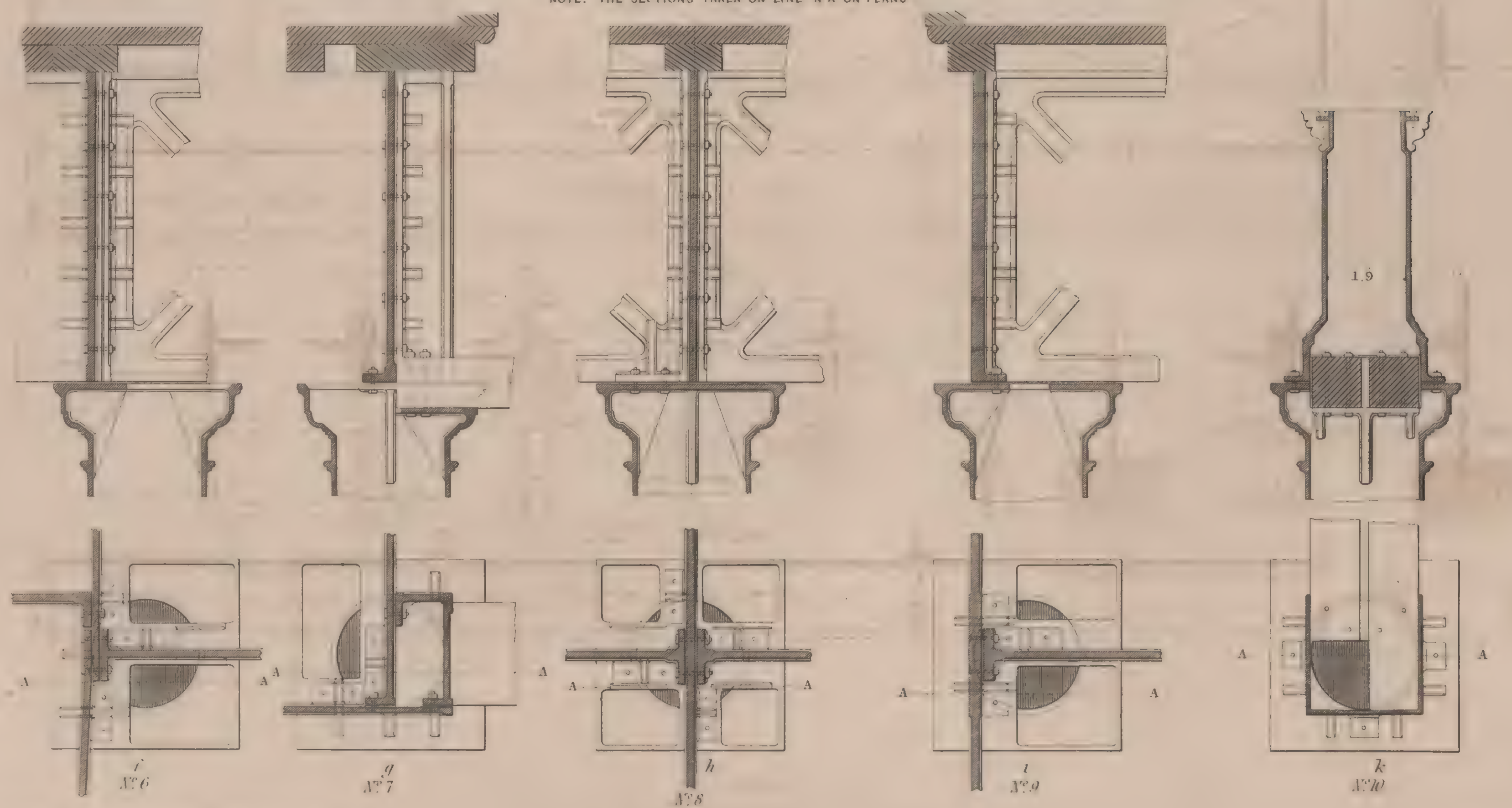
GRAVESEND PIER.

W. TIERNEY CLARK.





DETAILS OF PLANS AND SECTIONS OF CAPS OF COLUMNS AND DEEP IRON BEARING BEAMS
OF T HEAD, SHEWING FLANGES & BRACKETS WITH FASTENINGS AND MODE OF SECURING TIMBERS OF LANDING.
NOTE. THE SECTIONS TAKEN ON LINE A A ON PLANS



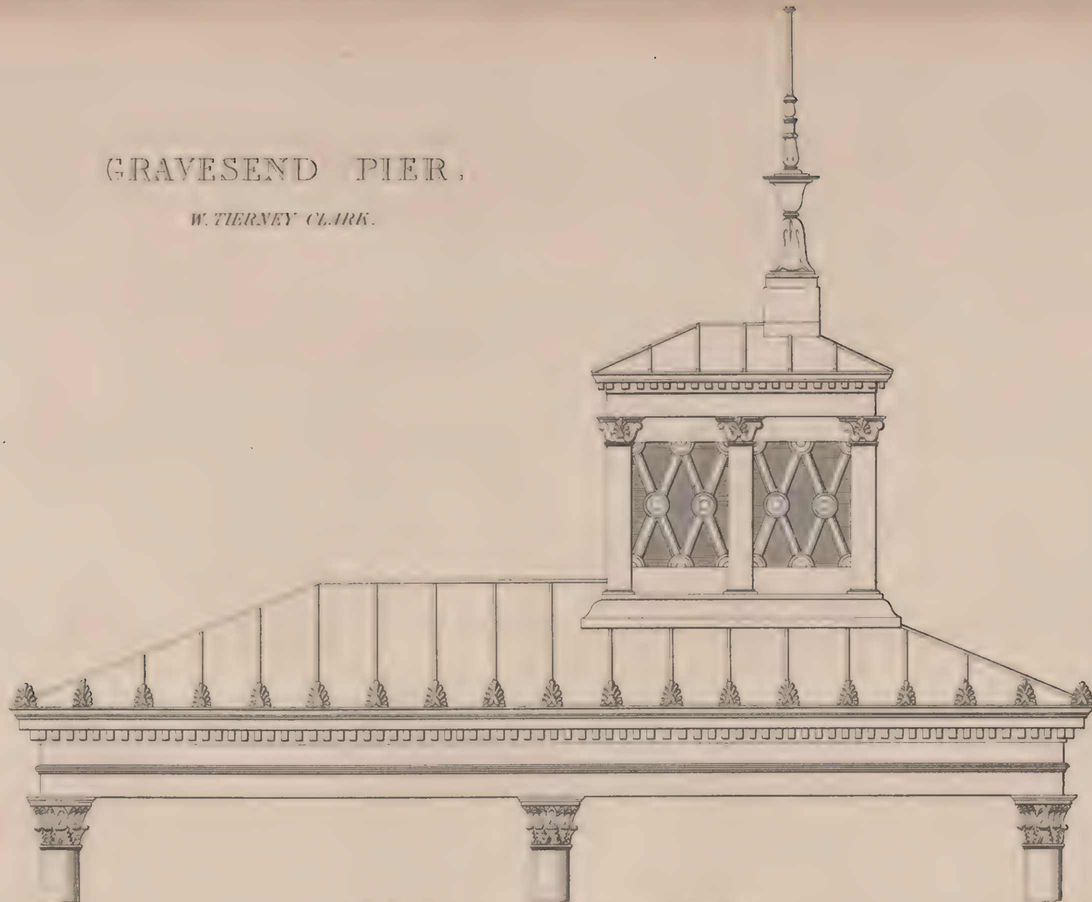
Reduced from the original drawings by Fred^k. Rumble

J. W. Lowry sculp

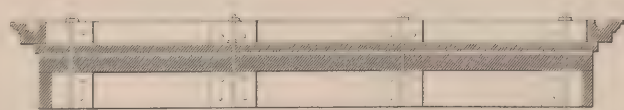
Institution of Civil Engineers 1840.

GRAVESEND PIER,

W. TIERNEY CLARK.

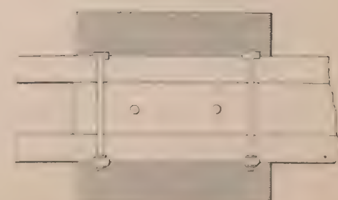


ELEVATION OF AWNING AND TURRET



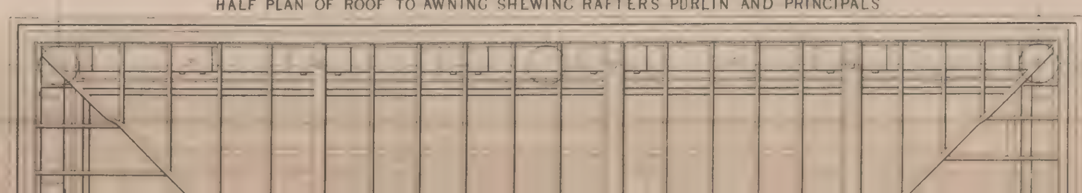
TRANSVERSE SECTION THROUGH TIMBERS &c.
FORMING ENTABLATURE AND CORNICE

MANNER OF SECURING THE TIMBERS
AT THE ANGLES

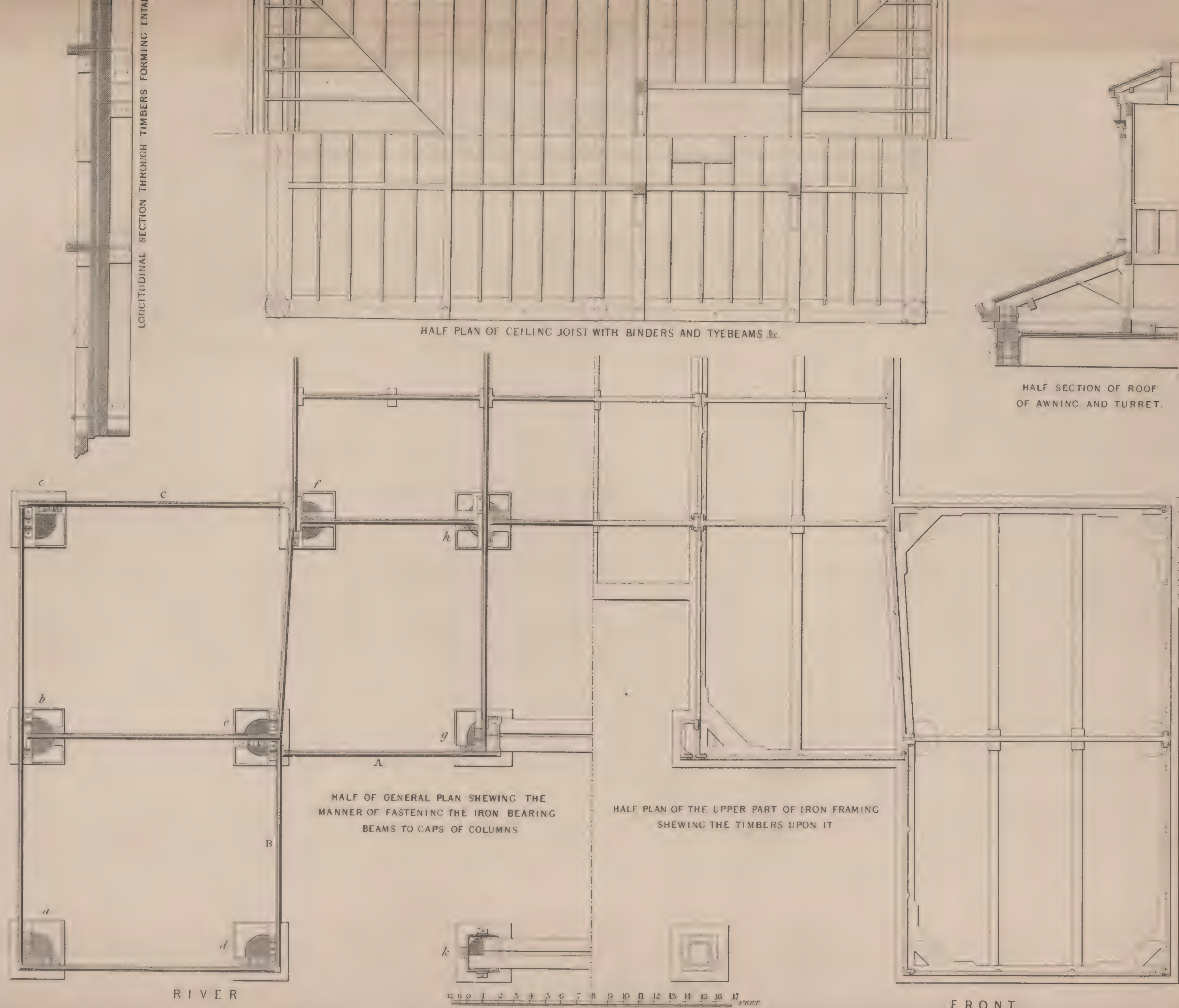


LONGITUDINAL SECTION THROUGH AWNING AND TURRET

HALF PLAN OF ROOF TO AWNING SHEWING RAFTERS PURLIN AND PRINCIPALS



NG ENTABLATURE AND CORNICE &c.



Reduced from the original drawings by Fred^k Rumble.